SCIENTIFIC AMERICAN SUPPLEMENT. No 1515

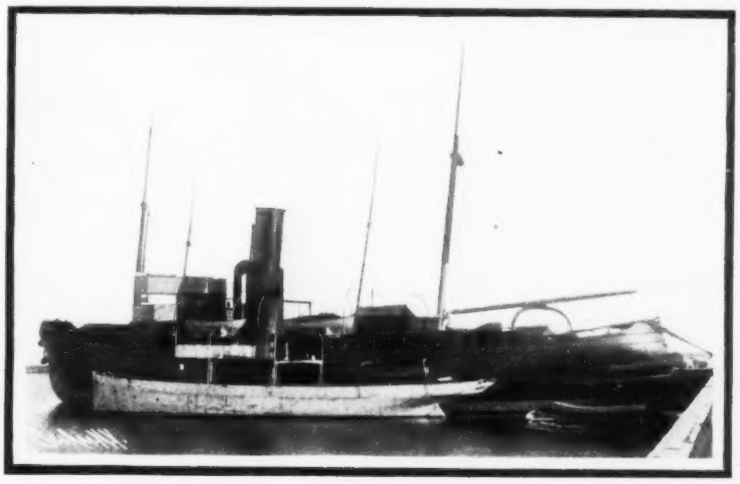
Entered at the Post Office of New York, N. Y., as Second Class Matter. Copyright, 1905, by Munn & Co.

Scientific American, established 1845.
Scientific American Supplement, Vol. LIX., No. 1515.

NEW YORK, JANUARY 14, 1905.

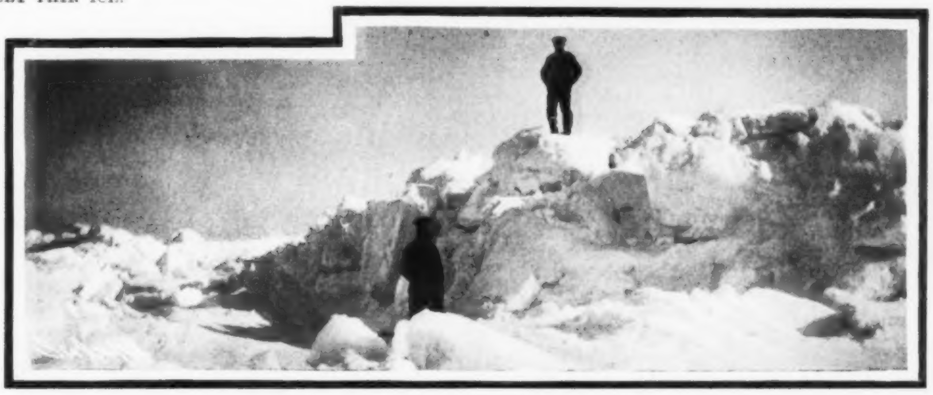
Scientific American Supplement. $5 a year.
Scientific American and Supplement. $7 a year.

TUG WITH WOODEN ICEBOW FOR WINTER USE AT PHILADELPHIA. A VERY CRUDE BUT SERVICEABLE ICE-BREAKER FOR COMPARATIVELY THIN ICE.



ICE-BREAKER "SLEIPNER" OFF COPENHAGEN; YACHT IN THE FOREGROUND.

"ERMACK" JAMMED IN THE ICE FOR THREE WEEKS.



AN ICE PACK IN THE GULF OF FINLAND.

ICE-BREAKER AT NIERLIAWK.

ICE-BREAKERS AND THEIR SERVICES.

ICE-BREAKERS AND THEIR SERVICES.*

By ARTHUR GULSTON.

IN the Northern Hemisphere of the world, there are many ports which during winter become closed to navigation, and choked with ice owing to climatic conditions. To free these ports, and render navigation possible throughout the winter, resort has been made to a class of steamers called "ice-breakers." These are of many forms and dimensions, varying from a steam launch 40 feet long, spoon-shaped at the bow like a Norwegian "pram," to the enormous "Ermack," of 8,000 tons displacement and 10,000 horse-power. These ice-breakers are placed on their various stations to assist steamers of the commercial fleets, which become fast in the ice, and which, without assistance, are unable to free themselves, owing—in most cases—to insufficient horse-power, and the light scantlings of their propellers. These vessels also run considerable risk of sinking, owing to the ice squeezing and holing their sides.

COPENHAGEN ICE-BREAKER CRASHING THROUGH THE ICE.

In this paper, it is intended only to deal with vessels that work among ice that is formed from year to year.

There are in Canada and in the Baltic many merchant steamers which can cope with ice of considerable thickness, and the ice-breaking mail boats of Helsingfors are fine examples of this type; but without the aid of "ice-breakers" proper, the work of the commercial fleets in winter would indeed be sadly crippled.

Railway ferry ice-breaking steamers are another type, and they play an important part in keeping open railway communications where bridges or viaducts are more or less an impossibility.

The first recognized "ice-breaker" was a tug called the "Pilot," of Cronstadt, belonging to a Russian gentleman named Britneff, who wished to keep the ice from fastening inside and outside the breakwater at Cronstadt, and to prolong the trade with lighters to St. Petersburg at the setting in of the winter for a longer period than had previously been possible. With this in view, a new bow was built on the vessel, and in 1870 she was set to work. She was also used to keep a canal open through the ice between Cronstadt and Oranienbaum, the present railway terminus on the coast, during the time of the formation of the ice. Before this vessel began to work communication to and from Cronstadt and the mainland was impossible for some time during the formation and breaking up of the ice. This boat had a single engine of 85 horse-power, and was about 65 feet long.

The "Pilot" was superseded in 1889 by two small ice-breakers, the "Zarja" and the "Luna," which act as ordinary tugs during the open season, and have a horse-power of 150 each, are 98 feet long, and draw 6 feet of water. When working in ice these two ice-breakers work together, each vessel running alternately upon the ice, and returning. This system of ice-breaking is necessary, as the mainland shore water is so shallow that a large ice-breaker cannot be employed on this station.

Hamburg and Copenhagen followed the lead of Cronstadt, and at Hamburg there are now several ice-breakers which have proved themselves capable of dealing with the heavy traffic in the ice between Hamburg and Cuxhaven during most of the winters that they have been at work. Though these vessels are not large, they are well designed and suitably arranged for the work they have to perform.

The first boat ("Eisbrecher No. 1"), built in 1871, is 130 feet, draws 13 feet, has 300 I. H. P., and has a spoon-shaped bow. The succeeding boats have increased in size and power, the last new one ("Eisbrecher No. 3") being 140 feet long, with a draft of 12 feet forward and 16 feet aft in working trim. Her engines are of the triple expansion screw type of 950 I. H. P. As a result of experience the form of "Eisbrecher No. 3" differs considerably from that of "Eisbrecher No. 1."

At Copenhagen the ice in the sound is during the hard frost very dense and much under-shot. In strong winds it packs tightly, and under some climatic conditions becomes spongy and difficult to negotiate.

The first ice-breaker at Copenhagen was the "Staekodder," 150 feet long, having only 10 feet 6 inches draft. She was 800 I. H. P., which was too little for a vessel of her dimensions to successfully negotiate the ice at this station, and her propellers were too near the load line, and suffered many accidents. This vessel is now stationed at Korsoer, and in winter assists the railway ferry steamers on the Korsoer-Nyborg route.

At the present time the fine ice-breaker, "Slejpner," belonging to the harbor authorities, controls the winter traffic in Copenhagen. She is a vessel of 2,000 I. H. P., and 1,450 tons displacement; is 161 feet 6 inches long, and her speed on trial was 12.75 knots. This vessel is fitted with compound machinery on the assumption that less damage was likely to occur to low-pressure boilers than would be the case with higher pressure owing to the constantly changing steam pressures consequent in an ice-breaker when at work. She is fitted with a lowering tube in the stern, to enable the propeller blades to be changed while afloat. The frames at the bow are close-spaced, and in fact the framing throughout the structure is of a strong character.

There is also another ice-breaker here, the "Bryderen," belonging to the United Steamship Company. This vessel is 131 feet long, has great towing power, and is of immense assistance to the large fleet of steamers belonging to her owners. She is used a great deal during the winter in Danish waters, to keep the communication open for the company's steamers between the ports where they run.

The services of these ice-breakers have often to be supplemented by the Copenhagen salvage steamers, owing to the large winter traffic in the Belts and Sound, where the ordinary cargo boat is at the mercy of the ice when she gets among it. Before the "Slejpner" and the salvage boats were put to work considerable delay occurred to steamers passing the Belts in winter and using Copenhagen during times of hard frost. The salvage boats are strengthened to resist ice.

At Korsoer, in the south of Denmark, three ice-breakers are used during the winter to break the ice in the Storr Belt, to keep the passages open for the important railway service between Korsoer and Nyborg, and, when the ice is too heavy for the railway ferry boats to pass through, these ice-breakers transport the mails and passengers between these ports. They are the "Tyr," the "Mjolmer," and the "Staerkodder;" the latter was originally at Copenhagen. The "Tyr" is 138 feet long, has compound engines, and is most efficient as an ice-breaker. The "Mjolmer" is 143 feet long, has compound engines, of 500 horse-power, and is an able boat among ice.

In the davits of these vessels are stowed what are termed "ice-boats," which are used for mail and passenger transport when the ice is too heavy for these vessels to work. These boats have runners under them, and are pulled over the ice by fishermen; the passengers walk, their luggage being in the boats, and when they come to an opening in the ice, all embark and cross the water, and resume walking on the other side. This method of traveling is exceedingly slow and fatiguing, but it was quite customary at one time, especially before the Danish State Railway in 1894 obtained a powerful ice-breaking railway ferry steamer.

There are also ice-breakers at Kiel, Riga, Rostoff, Stettin, Libau, Amsterdam, Kalmar, Stockholm and Nicolaisk, but they are all more or less vessels similar in type to the "Sampo" and "Ledokol III.," built on the River Tyne. There are many small ice-breakers used as post boats and pilot boats in the Baltic, the German coast, and the Black Sea. At Amsterdam, the ice in the Sea Canal is kept in movement principally by small steamers, and, as it is not very thick, this arrangement answers well in ordinary winters.

Nearly all the canals in Holland, when frozen, become the principal means of communication by skating and sledging from one town or village to another.

Ice-breaking as an assistance to commerce is brought to a high state of perfection at the ports of Hangö and Helsingfors, in Finland, though the latter port is not kept open all the winter, the whole power of the Finnish ice-breakers "Murtaja" and "Sampo" being, about January, concentrated entirely on Hangö. The latter vessel was built on the River Tyne. She has a propeller at each end, the bow engine being 1,200 I. H. P., and the stern engine 1,350 I. H. P. She is 202 feet long, and 2,000 tons displacement; has a draft of water of 18 feet, with all coal and stores on board, and her sides are angled considerably. She was put to work at the commencement of the winter of 1898, and has been most successful during her career.

The actual performance of the "Sampo" in the Gulf of Finland shows that she has a very efficient form for ice-breaking; she passes through field ice of 12 inches to 16 inches in thickness at eight knots an hour. The thickness of the drift ice she has been generally at work in, is some 8 to 10 feet, through which she can pass at from two to three knots an hour. She also breaks down larger packs without much trouble. It is found in practice with this vessel, that charging in drift ice is more effective than cutting it down with the bow propeller, as is advocated in America.

The "Martaja" and the "Sampo" proceed out into the Gulf of Finland as far as the open water in the Baltic, breaking a canal, through which they afterward escort the cargo steamers into the harbor, the same operation being repeated when the vessels are ready to sail. In the Gulf of Finland, the harbors of Hangö and Reval are much affected by the wind, as the outer drift ice during a southerly wind makes Hangö difficult of approach, and a westerly or northerly wind has the same effect at Reval. During gales, the ice-breakers of these parts suspend operations, as the pressure of the ice becomes so enormous that even if the ice-breaker can force her way, the canal behind closes up almost at once, so that to the ordinary tramp steamer it becomes a problem as to whether it is safer to proceed or to come to rest in the ice, in which latter circumstance she will be drifted with the ice, or squeezed if the ice packs.

There are two ice-breakers at Gotheberg, and two at Drammen, in Norway; these vessels are well able to keep these ports open.

At Christiania, there is a fine ice-breaker, the "Isbjern," built at that port. She cuts a canal down the lovely fiord from the harbor to the sea, and keeps a channel free under all the conditions of ice at this place.

Ordinary merchant steamers trading during the winter in ice should have the plating at the bows doubled, and the side plating for some width about the waterline should be of a much heavier scantling than usual. The propeller blades should be of very strong design, and fitted to the boss to facilitate repairs; when this is done, steamers can take care of themselves to a very large extent. Without strengthening of this nature vessels dare not "charge" the ice, or the plating would be holed, and it is impossible to drive the engines, or the blades of the propeller would be knocked off. On the other hand, if the ship is allowed to rest in the ice, the sea inlets become frozen up, and the risk of being squeezed and holed is ever present.

Many German, Finnish, Norwegian and Danish steamers comply more or less fully with these requirements, and are therefore able to give a good account of themselves in ice.

ICE-BREAKING MAIL STEAMER "OIHONNA" GOING UP TO STOCKHOLM.

ICE-BREAKERS AND THEIR SERVICES.

* Read before Society of Arts.

At Reval there are four ice-breakers of varying dimensions, and the efforts of these vessels are supplemented by a well-arranged system of telegraphing all ships approaching the coast. The larger ice-breaker attends to vessels to and from the harbor to the entrance of Reval Gulf, a distance of 15 miles, and occasionally goes as far as the open water beyond. The three smaller boats keep the ice broken inside the mole and harbor, and tow and move the vessels in the harbor.

The large boat is a fine vessel, but somewhat out of trim, as when she was put to work it was found that she pushed the broken ice in front of her, owing to her bow lines being too full. To meet this difficulty she is trimmed a good deal by the stern, but this prevents her having the valuable advantage of "tipping" herself more by the stern when she sticks on or in the ice. An ice-breaker should be capable of altering her trim quickly by moving water from her forward end to the after end, or by filling up the after compartment rapidly, as should she stick forward her engines might be unable to release her.

At Odessa there are three powerful ice-breakers, the latest one, "Ledokol III.," having been built at Walker. She is 148 feet long, 2,200 I. H. P., and has a speed of 13 knots. This vessel is well able to break the ice three feet thick and has, therefore, been most successful on her station, never having been jammed and being always able to free others in distress or fast in the ice. The ice at Odessa is principally pack ice of a broken-up description, which at times owing to wind and tide packs very tightly, and occasionally freezes together in a hard, compact mass.

At Vladivostock, in Eastern Siberia, there is a fine ice-breaker, the "Nadeshny," which is of vast importance and assistance to the port, as the Russian fleet stationed at Port Arthur is now able to patrol this harbor, which is the naval base, during the whole winter. The ice at Vladivostock attains a thickness in hard winters of 36 inches, but the packs only occur at the outer edges of the field ice, when the wind blows in the direction of the entrance to the harbor. The harbor is an ideal one surrounded by hills, is quite land-locked, and has a curved entrance from which the town cannot be seen.

The "Nadeshny" was built at Copenhagen, is 183 feet long, has twin screws of 2,800 I. H. P., and is fitted with powerful pumping appliances that can be used for salvage purposes. The vessel has a speed on trial of 14.2 knots in open water.

Coming now to the design of ice-breakers and vessels suitable for working in ice, they should have the bow angles and lines so arranged that when they have mounted the ice, and the ice is giving way under the vessel's weight, they must not jam when returning to be water-borne forward, always remembering that they are advancing and should remount the ice. When the ice is broken down it should pass along below the vessel or under the field ice, otherwise it lies on the water and has a tendency to jam the vessel sideways; this results in the ice-breaker having to smash a larger proportion of ice than necessary, to give side clearance, thus absorbing more power, coal and time, and probably resulting in having to back and charge the ice. An ice-breaker should also be able to turn out of the channel she has cut, and the form of the bow lines has much effect on this maneuver.

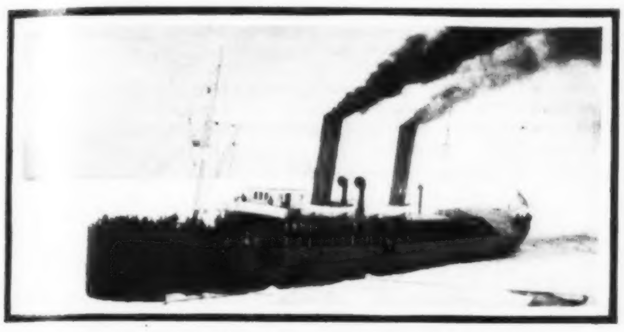
The designs of ice-breakers vary so much that there are no certain data to guide builders; but practice has shown that the full forward form of spoon-shaped bow is not successful in hard and packed ice, as the vessel pushes the ice in front of herself, instead of cutting and dispersing it. It should be borne in mind that ice-breakers when "charging" in heavy ice, are in collision, so to speak, during the whole time that they are at work; this, therefore, entails much more strengthening at the bow and sides as the ice to be dealt with becomes more formidable. The shell plating must be considerably increased in small boats, and still more so as the vessels increase in size; additional stringers, stronger decks, and a liberal addition to the number of bulkheads, transverse and longitudinal, as well as many pillars, become a necessity to prevent constant recurrence of repairs.

In the days before ice-breakers came into use at Amsterdam, Stockholm, and other ports, the custom was to have from 50 to 200 or more men to liberate a vessel. These men formed two parties, making holes through the ice at a sufficient distance apart to clear the beam of the ship, then from hole to hole a groove was cut, and the ship would charge to the best of her ability to break the ice, or saws were used to cut from hole to hole, but the operation was very slow and tedious. From a fortnight to six weeks to go fifteen miles to the open water from the ports of Reval and Amsterdam, was a fair time for a vessel, with success, to clear the ice.

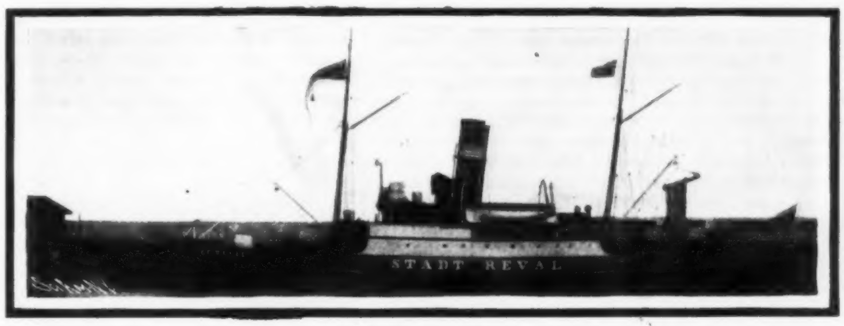
In arranging the accommodation in ice-breakers, it is desirable to have all this under the weather deck, for the sake of warmth and comfort, with covered-over companions forward and aft for access below. All piping should be kept under the deck, and the fire pipes should be fitted with hydrants below and above this deck. The boiler rooms must be well closed up, and consideration has to be given to the disposal of the ashes, and special arrangements have to be made inside the vessel on which the sea inlets are fitted. Steaming arrangements and circulating water should be delivered at will to the sea inlets, to warm the circulating water, and prevent ice forming in severe frost.

Care has to be given to the lining of the cabins at the ship's sides, and it has been found that air spaces are the best non-conductors of cold. Condensation is always a difficult problem, and dried heated air pumped in lends itself better to overcoming this trouble than the ordinary system of steam heating. Ventilation has also to be carefully considered, though it is not so complex a question as that of heating.

The rudder should be arranged for easy unshipment afloat, and should be of large area and immensely strong. The moving parts of the machinery and the shafts must be extra strong and largely in excess of ordinary practice. Large surface in the stern tubes is also required to support the shafts when the blades are striking the ice and smashing it up, and the boss and blades must be of the most liberal dimensions. The vessel should be so designed that, if possible, she can be "tipped" to replace a propeller blade while afloat.



ICE-BREAKER "ANGARA" AT WORK ON LAKE BAIKAL.



THE "REVAL."

THE RUSSIAN ICE-BREAKER "ERMACK." CAPABLE OF FORCING HER WAY THROUGH ICE TWENTY-FIVE FEET THICK.

ICE-BREAKERS AND THEIR SERVICES.

On the engines it is preferable to have steam reversing gear, as the "all-round" type is a very heavy tax on the engineers when the ship is ice-breaking. In fact all the controlling gear of the engines, if they are large, should be mechanically worked so as to reduce to a minimum the labor entailed by the almost continuous handling of the engines when the ship is at work.

The safety valves should have silent blow-offs to the condenser so that at sudden stoppages, commands on deck can be given to the crew, or to another vessel alongside of which the ice-breaker may be at the moment. Noise occasioned by escaping steam is most annoying, and makes it nearly impossible to transmit orders on deck.

Most ice-breakers are fitted with large pumping arrangements for salvage purposes, as cargo steamers often get damaged in the ice and require assistance to keep afloat.

(To be continued.)

[Continued from SUPPLEMENT No. 1513, page 24242.]

EXPERIMENTAL ELECTROCHEMISTRY.*

By N. MONROE HOPKINS, M.Sc., Ph.D.

Assistant Professor of Chemistry in the George Washington University, Washington, D. C.

FOURTH PAPER.

Novel Experiments in "Electrolytic Induction."

As it is the purpose of the first few chapters of this work to acquaint the student with the constitution, and

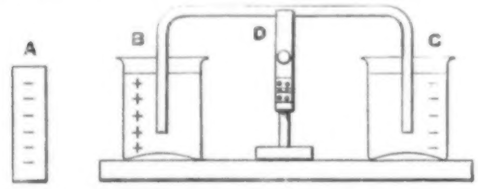


Fig. 1.—Prof. Ostwald's Experiment in Static Induction to Show the Presence of "Free Ions." A. Negatively-charged Body. B and C. Beakers Filled with a Solution of Potassium Chloride. D. Siphon Tube Filled with the Same Solution and Joining the Two Beakers.

behavior of electrolytes under various conditions, such effort would fail without touching upon the electro-static and electro-magnetic deportment of substances in solution. Having dealt with electrolytes theoretically and experimentally, and learned the fundamental laws upon which their behavior depends, we will be in a position to take up the practical work which is to follow in the later chapters, and from the subject of electrochemistry as a science touch upon electrochemical engineering as an art. We are, therefore, acquainting ourselves with electrolytes, the theories upon which they are based, and their capacities as electrical conductors. We shall also have electrolytes as producers of the electric current, but this phase of substances in solution is best left until a little later. The first experiment illustrating the effects of electrical induction upon an electrolyte as given in Fig. 1 was designed by Wilhelm Ostwald, Professor of Chemistry in the University of Leipzig, and one of the most distinguished physical chemists Germany has ever produced. Prof. Ostwald's experiment has for its object to prove the existence of "free ions" in an electrolyte, and to show that they actually migrate and carry the electrical charges upon them. The author became much interested in Ostwald's work, and repeated the experiments for himself, continuing the research still farther, as will be described in the present chapter, developing what may be termed "electrolytic induction." Let us first take up the experiment of Ostwald and refer to the illustration. In the experiment with potassium chloride, Ostwald writes as follows: "The following consideration may serve to remove the last

Fig. 2.—Ostwald and Nernst's Experiment in Static Induction to Show the Presence of "Free Ions." A. Positive Knob of Electrical Machine. B. Glass Flask Covered with Tinfoil. C. Wet Strings Connecting the Glass Flask and the vessel D, Both Containing Dilute Sulphuric Acid. E. Burette Drawn Out Into a Fine Capillary G, Through the Side of which the Platinum Wire, F, is Fused. H. Glass Plate on Glass Insulators.

doubts as to the validity of the assumption of free electrically charged atoms of chlorine and potassium. Imagine two insulated vessels, *B* and *C*, filled with a solution of potassium chloride, and electrically connected by means of the siphon *D*. Let a negatively charged body be brought near *B*, remove the siphon, and lastly the charged body *A*. Then, as is well known, *B* remains positively electrified, and *C* negatively electrified. Now, according to Faraday's law, the electricity in electrolytes can only move simultaneously with the ions. Consequently, if an excess of positive electricity is present in *B*, there must also be an excess of free potassium ions, i. e., of potassium atoms, by the electricity of which the charge is determined. If the electricity is conducted away,* the potassium assumes the ordinary form, and acting on the water of the solution develops hydrogen, which can be collected in suitable apparatus and tested. Similar considerations hold good for the chlorine in the vessel *C*. It is consequently not only conceivable that the ions in an electrolytic solution move about with electrical charges, otherwise quite free, but solutions may be prepared which contain an excess of any ion we choose, e. g., an excess of potassium. The assumption that electrolytes contain free ions is not only possible, but necessary."

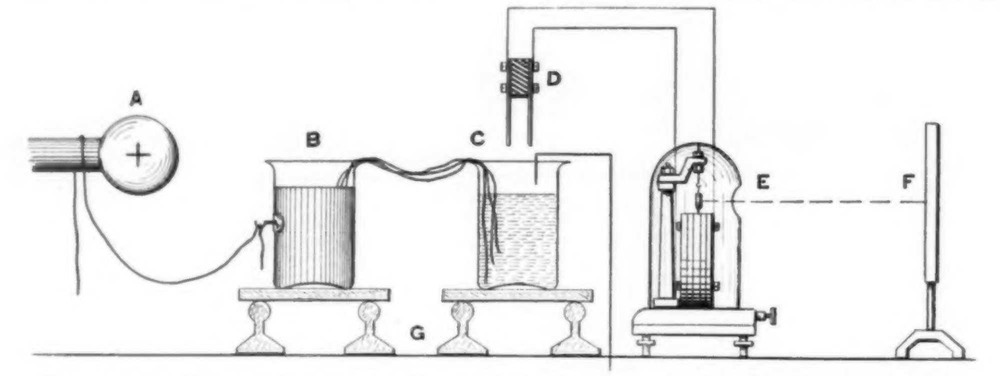


Fig. 3.—Hopkins Experiment in Static Induction to Show the Presence of "Free Ions." A. Positive Knob of Electrical Machine Joined to Tinfoil Covering of the Beaker, B, by Means of a Small Metal Chain. C. Similar Beaker Without Tinfoil Covering, but Insulated on Glass at G. Both Beakers are Filled with Potassium Chloride Solution and are Joined by Wet Cords. D. Platinum Electrode Attached to Hard Rubber Insulator. E. Delicate Reflecting Galvanometer. F. Scale of Reflecting Galvanometer.

This experiment as originally proposed by Ostwald was not at all practical, for the quantity of hydrogen gas liberated was so small that it could not be seen.

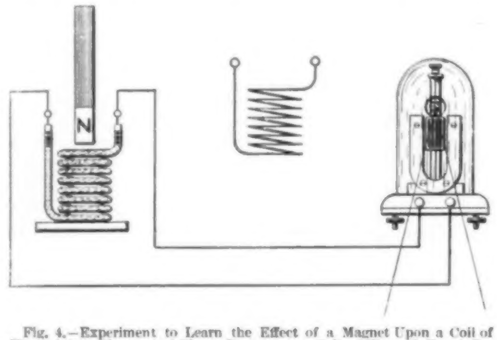


Fig. 4.—Experiment to Learn the Effect of a Magnet Upon a Coil of Electrolyte. The Central Figure Represents a Coil of Wire of Equal Resistance and Dimensions, which May be Substituted for the Coil of Electrolyte.

The liberation of hydrogen is based upon the following simple equation:

$$2K + 2H_2O = 2KOH + H_2.$$

The experiment was eventually modified by Profs. Ostwald and Nernst, the latter being also one of the most brilliant German physical chemists of the times. This experiment shows to the eye the liberation of hydrogen under similar conditions of static induction, and is a practical illustration of great beauty. The arrangement of the apparatus for this experiment is shown in Fig. 2. At *A* we have the positive knob of a static electrical machine, connected by a tinsel cord or small metal chain to the little hook on the tinfoil covering of the glass flask *B*. This flask is filled with dilute sulphuric acid and is thoroughly insulated upon a glass, or hard rubber plate resting upon little insulators also of glass. Cords or strings wet with the same dilute sulphuric acid dip into the flask and connect with a vessel *D* also containing some of the same sulphuric acid solution, and being insulated in a similar manner. The glass burette *E* has been drawn out into a long and fine capillary *G* through which a fine platinum wire is fused, and which runs to earth. Now what happens when the electrical machine is put into operation? The tinfoil coating being electrically-connected with the electrical machine becomes positively charged, which, acting through the glass of the flask, attracts and holds a corresponding amount of negative electricity, while the positive is repelled. The positive electricity, or, as we believe, the positive ions, which in this case are hydrogen ($\overset{+}{H_2}\ \overset{-}{SO_4}$ ionizes into $\overset{+}{H_2}\ \overset{-}{SO_4}$) is repelled through the moist cord which leads to the vessel *D* and the capillary of the burette filled with the acid and water to a height of a few centimeters, when it meets with a little column of mercury at *G* connected to earth. This mercury was drawn up into the capillary by placing it in the bottom of the vessel *D*, when some of the dilute sulphuric acid solution was allowed to follow. Now the hydrogen ions are repelled through this system and are discharged when they reach the grounded mercury. They then become ordinary atoms of hydrogen, and may readily be seen in the capillary. On starting the electrical machine the experimenters observed a rush of tiny bubbles of gas through the mercury at *G*, collecting at the top under the glass stop cock. The $\overline{SO_4}$ ion being held by the positive attraction on the outside of the flask *B*. Here we have a very beautiful experiment based upon an induction phenomenon. The experimenters also conducted a most elaborate quantitative research upon this phenomenon, to ascertain if the amount of hydrogen set free at *G* corresponded to that calculated from Faraday's law, and found within the limits of experimental error, that it did. We shall take up Faraday's law and the beautiful subject of electrochemical equivalents in a later chapter, but at the present time it is only wise to state that all ions have definite capacities for the electrical charges according to their valencies. Knowing the electrochemical equivalent of hydrogen, for example, it would be an easy matter to calculate what mass, or what volume of hydrogen, would be set free by a given quantity of electricity. The experimenters referred to employed such a course in checking the above experiment quantitatively. So much for the experiment of Ostwald and Nernst, depending upon the liberation of hydrogen as proof of the migration of free ions. Fig. 3 illustrates the author's modification of this beautiful experiment based upon the reflecting galvanometer as a chemical indicator, to prove that ions had migrated under the influence of static induction. Gaugin, and later Prof Kuester, employed the reflecting galvanometer as a chemical indicator, and the use of such an instrument for detecting acids and bases will be described before we describe the author's experiment. To quote from Gaugin's work we have the following: "Electromotive Force of Platinum in Contact with Acidulated or Alkalinized Water: Two plates of platinum were immersed in acidulated water for some time. One be-

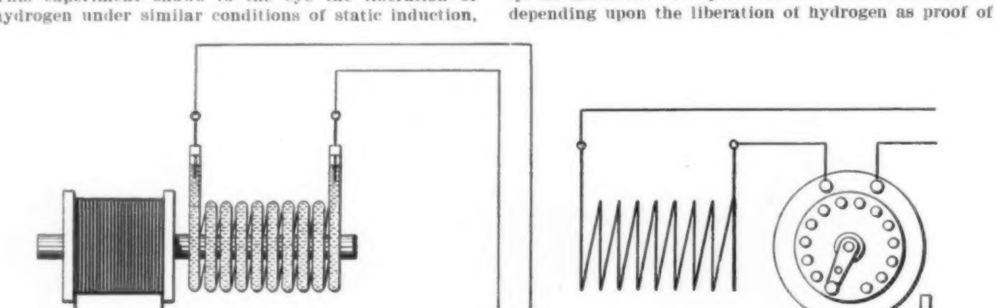


Fig. 5.—More Refined Method of Determining Quantitatively the Effect of a Magnet Upon a Coil of Electrolyte Compared with a Coil of Wire of Equal Dimensions and Equal Ohmic Resistance.

* Specially prepared for the SCIENTIFIC AMERICAN SUPPLEMENT.

* By inserting in the beaker *B* a platinum wire to earth.—N.M.H.

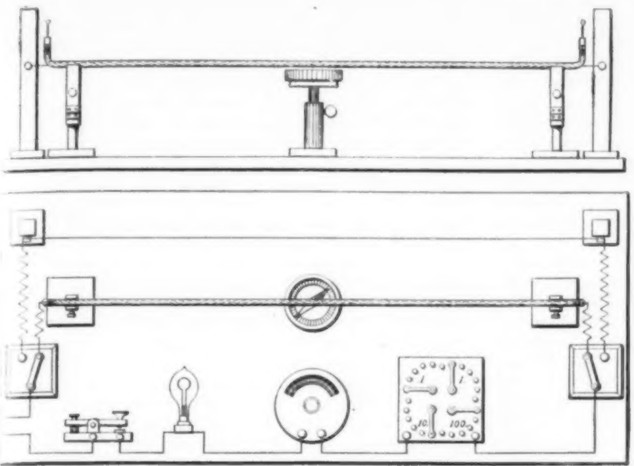


Fig. 6.—Experiment to Show the Effect of Electrolytic Conduction Upon a Magnetic Needle. The Experiment is so Designed that a Wire Carrying an Equal Current of Electricity May be Substituted for the Electrolyte, and the Deflection of the Magnetic Needle Quantitatively Compared. We Can Throw in Series with the Electrolyte or Wire at Will the Lamp, Ammeter and Variable Rheostat which Join to a Lighting Circuit or Storage Battery. The Arrangement of the Two Switches at Either End will Make this Clear.

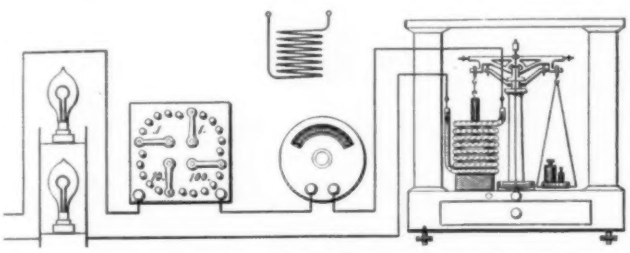


Fig. 7.—Experiment to Show and Quantitatively Measure the Magnetic Pull of an Electrolyte Carrying an Electric Current. At the Left is an Ammeter, a Variable Rheostat, and a Lamp Bank. Above the Ammeter is a Coil of Alloy Wire for Substitution Purposes.

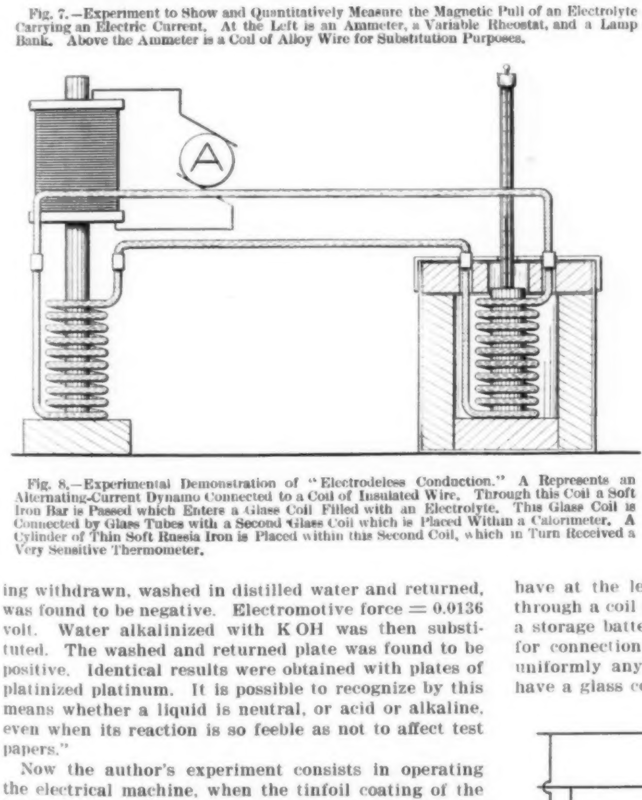


Fig. 8.—Experimental Demonstration of "Electrodeless Conduction." A Represents an Alternating-Current Dynamo Connected to a Coil of Insulated Wire. Through this Coil a Soft Iron Bar is Passed which Enters a Glass Coil Filled with an Electrolyte. This Glass Coil is Connected by Glass Tubes with a Second Glass Coil which is Placed Within a Calorimeter. A Cylinder of Thin Soft Russia Iron is Placed within this Second Coil, which in Turn Received a Very Sensitive Thermometer.

ing withdrawn, washed in distilled water and returned, was found to be negative. Electromotive force = 0.0136 volt. Water alkalinized with K OH was then substituted. The washed and returned plate was found to be positive. Identical results were obtained with plates of platinized platinum. It is possible to recognize by this means whether a liquid is neutral, or acid or alkaline, even when its reaction is so feeble as not to affect test papers."

Now the author's experiment consists in operating the electrical machine, when the tinfoil coating of the beaker *B* will be positively charged and will hold the negative ions of the potassium chloride, $\overset{+}{K}\ \overset{-}{Cl}$, which as may be readily seen are chlorine ions, and will repel the positive ions which are potassium, through the moist cord into the beaker *C*, where they may be discharged after the removal of the wet cord, by the platinum wire shown at the right of the beaker. Upon discharging the potassium ions they become potassium atoms, and react with the water as before $K + H_2O = 2KOH + H_2$, forming potassium hydroxide and setting hydrogen free. Now the present experiment does not attempt to show migration by the setting free of the hydrogen, but by the formation of the alkali, or base, KOH, potassium hydroxide. To do this the reflecting galvanometer is employed. It would be expected that the chlorine ions could be discharged in the same manner and their presence shown by a drop or two of silver nitrate solution. Although there is little doubt of their being discharged in the same manner, the minute quantity of chlorine present would not suffice to give a chemical precipitation of silver chloride. Perhaps if the electrical machine was allowed to run for several days, a slight opalescence might be observed when a drop or two of silver nitrate is added. When we complete our studies of Faraday's law involving the electrochemical equivalents we will be in a position to appreciate how few chlorine ions would migrate under such circumstances as we have in this experiment. All ions carry very great electrical charges, and we know as physicists, that there is very little quantity of electricity to be had from a static machine. The electricity from a frictional machine is *almost all potential difference!* The amperage in a current from a static machine is so small as to be detected and measured only by very special means. Now a few ions are capable of carrying many amperes, as we shall see later, and it is not surprising, under the circumstances, that our static charges have been carried by very few ions indeed. We will now leave experiments with static induction, and study the effects of magnetic and galvanic induction upon electrolytes. All the following experiments are based upon original research of the present author, and are now published for the first time. It occurred to the writer to compare electrolytes with metallic conductors under the influence of magnets and electric currents in neighboring conductors, to see if inductive effects and inductive currents were produced. Will a magnet induce a current of electricity into an electrolyte as it does in a metallic conductor? This was a question not touched upon in the treatises in physics or chemistry, and it was therefore resolved to answer the question by experiment. Fig. 4 shows the first comparatively rough plan for learning whether a magnet will induce an electric current in a coil of electrolyte as it does in a coil of wire. We have here a sensitive reflecting galvanometer at the right to show any induced current. As a matter of fact a magnet does induce a current of electricity in the electrolyte, and causes the galvanometer to indicate the same. The little coil of wire represented in the center was made of equal dimensions, and was substituted for the coil of electrolyte to ascertain if the effect was quantitatively the same. The coil of electrolyte consists of a glass tube filled with a dilute solution of sulphuric acid. It was necessary to introduce in series with the metal coil some additional resistance, which was of non-inductive type, in order to obtain comparable conditions, as the coil of electrolyte has a much higher ohmic resistance than the coil of wire. The deflection of the galvanometer proved to be the same in both cases. As it was an impossible matter to place the magnet into the two respective solenoids in exactly the same manner, and in exactly the same time, the experiment as illustrated in Fig. 5 was conducted. Here we have at the left a soft iron bar running horizontally through a coil of insulated wire which is in series with a storage battery, the terminals of the wire being free for connection to a contact key which may be closed uniformly any number of times. Next to the coil we have a glass coil filled with any good electrolyte in solution, into which the terminal wires (which must be of platinum) of a reflecting galvanometer dip. At the extreme right we have a coil of resistance wire of equal proportions and equal number of turns as we have in the glass coil, and in series with it a rheostat of non-inductive type, for bringing the wire to the same resistance as the coil of electrolyte. Of course some wire of high resistance must be used, such as is employed in resistance sets, in order that we will not have to depend upon much outside resistance, as by the use of the rheostat. The coil of alloy wire may now be substituted for the coil of electrolyte, and by means of the key and storage battery, operating the electro-magnet, we can produce the same number of magnet lines of force in just the same way and in the same time as we did in the case of the coil of electrolyte. Experiments with such a piece of apparatus gave the same deflections of the galvanometer with a coil of electrolyte as they did with a coil of alloy wire. We can then think of the free ions being migrated by ordinary magnetic induction so common to all students of physics and electrical engineering. Let us now study the effect of an electric current traversing an electrolyte, upon a magnetic needle. For this purpose set up a piece of apparatus like that represented in Fig. 6. Here we have a glass tube about a meter in length by about a centimeter in internal diameter, bent up at the ends as indicated. This is filled with dilute sulphuric acid, and is provided with platinum electrodes with *loose-fitting stoppers*. The tube is supported on two laboratory stands above a delicate compass needle with a graduated arc or scale. There are also two upright standards provided with insulators between which an alloy wire is stretched taut at the same height as the glass tube, so one may be substituted for the other above the magnetic needle. The magnetic needle is represented upon an adjustable stand in order that it may always be brought to exactly the same distance below the wire and electrolyte respectively. The measurement must be made from the center of the wire and electrolytic tube respectively. Below in the same illustration we have a plan of the apparatus, looking down upon it. It will be readily seen with such an arrangement how the wire may be quickly substituted for the electrolyte, and how the electric current may be controlled, and made to flow through the electrolyte. In conducting this experiment the student will be impressed with the greatly superior conductivity of metals and even high resistance alloys over electrolytes. The result of such a carefully conducted experiment will show that the magnetic effect of electric currents traversing electrolytes is quantitatively the same as electric currents of equal strength traversing conductors of the first class. The thoughtful student will be likely to ask why the effect is just the same when we have positive ions going to the cathode carrying positive electrical charges and negative ions going to the anode carrying negative electrical charges. The only answer that can be given is that a negative ion traveling from right to left tends to turn the magnetic needle in the same direction as a positive ion does traveling from left to right. We know that the same current traversing a wire will turn a magnetic needle to right and left respectively, according to its position above or below the needle, and that we can greatly increase the magnetic effect by carrying the wire over and under the needle a number of times. We may say that a positively charged particle, or ion, produces the same effect upon a magnetic needle, traveling from right to left, as a negatively charged ion does traveling from left to right. The author has designed an elaborate experiment to show this by a rapidly running band of pure silk ribbon, upon which are pasted little tinfoil disks. The band may be run right-handedly under a suspended magnetic needle, with positive static charges upon the little

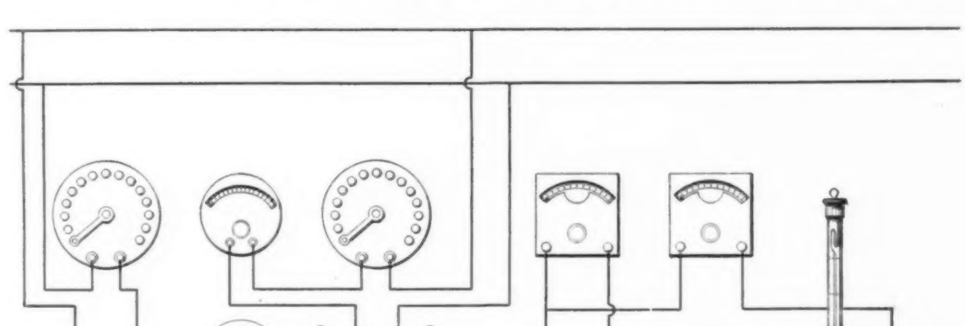


Fig. 9.—Apparatus to Study the Heating Up of Electrolytes by Alternating Currents at Various Frequencies. At the Extreme Right, Calorimeter, Electrolyte, Electrodes and Beckmann Thermometer. Alternating-Current Ammeter and Voltmeter in Connection with Leads. Alternating-Current Generator with Separately Excited Fields, and Rheostat for Varying Strength of Same. Electric Motor at Extreme Left in Series with Rheostat for Controlling Speed, and Ammeter for Reading Current Taken.

tinfoil disks, and the deflection and direction of the magnetic needle noted. The direction of the band may then be reversed, and the little disks be charged negatively, when the deflection and direction of the needle are again noted. The little disks are charged by passing under and touching a tinsel brush connected with either pole of an electrical machine of the static type. This is a mechanical representation of migrating ions in opposite directions. Owing to the small quantity charge of electricity upon the little disks which were placed about three centimeters apart, the ribbon was run at high speed. The drums over which the ribbon ran were supported upon solid glass axles to insulate the same. As a matter of fact ions travel very slowly, but carry very large charges of electricity. In the running ribbon we have very small charges of electricity, and therefore to obtain the same magnetic effects we would be obliged to drive the ribbon and little tinfoil disks at very high speed. The magnetic needle must therefore be protected from resulting air currents in some suitable manner. Having seen the effects of electric currents passing through electrolytes on magnetic needles, it remains only to see the effect of electric currents passing through electrolytes on masses of ordinary soft iron. Fig. 7 illustrates a simple experiment to measure the "magnetic pull" upon a soft iron bar, if such pull exists. At the right hand side of the diagram we have an analytical balance with the left pan removed in order that we may suspend the little bar of soft iron to be experimented upon. Directly under the little iron bar is a block of wood or other suitable support for the glass coil of electrolyte. The little platinum electrodes which dip into this electrolyte connect through an open scale, or delicate ammeter, in series with the variable rheostat and lamp bank. The lamp bank described in the first chapter of our series will of course be the best. We can now by this arrangement admit current to the electrolytic coil, read the current in amperes, and weight the magnetic pull to a great degree of accuracy. Substituting the alloy coil, we can, by means of the lamp bank and variable rheostat, cause the same current to flow through the alloy coil, and weigh the pull. As a result of scores of tests of this character, the author found the pull to be just the same with an electrolyte as it was with a wire carrying the same current. Of course it goes without saying that the convolutions, and consequently the ampere-turns were the same in both cases. For accurate work it must be impressed, however, that a rather fine wire of high specific resistance must be employed for the conductor of the first class. Otherwise the resistance of the wire coil will be so much less than the electrolyte that we must turn in a great deal of outside resistance through the agency of the rheostat. Perhaps the most interesting of all these experiments is that illustrated in Fig. 8 which has been termed a demonstration of "electrodeless conduction." Here we have simply a closed system, an electrolyte without the customary electrodes for giving and taking the electric current. Let us refer to the diagram and describe the method of showing this remarkable phenomenon. The illustration represents an original experiment of the author performed a number of years ago, but like those preceding, has never been published. *A* represents an alternating current generator connected to a coil of insulated wire on the spool which incloses a soft iron bar. This soft iron bar passes into a glass coil of tubing containing dilute sulphuric acid, and is joined through two straight glass tubes about a meter long to a second glass coil filled with the same solution. This second coil of electrolyte, however, is incased within a calorimeter made from a common pasteboard muff box, lined within with hair felt, as indicated by the diagonal lines. Within this coil is placed a small cylinder of thin Russia iron, which receives in turn and incloses the bulb of a sensitive thermometer, like those employed in our previous calorimeter work. One of Beckmann's thermometers with arbitrary scale and reservoir at the top is an excellent type. The cover is placed on the calorimeter, and after equilibrium has been established, the thermometer is read, and the dynamo started. The thermometer will slowly rise when within the little iron cylinder. If the cylinder is removed the mercury in the thermometer will fall again, and rise once more upon lowering the cylinder. What part does the little cylinder play? It is well known to all physicists and most electricians that iron heats up when it is magnetized first in one direction, and then in the other, as it is by the alternating current. This heating of iron by an alternating current under such circumstances is called "hysteresis." Here we have the heating of the little iron cylinder by being rapidly magnetized first in one direction and then in the other, which gives us proof that the closed system, without any electrodes whatever, is conducting the electric current. On breaking the system anywhere, with the dynamo still in operation, the heating ceases. Here we have undoubtedly the ions driven first in one direction, and then in the other, reversing their magnetic effect with their direction. If we could insert our thermometer in the electrolyte itself, we would probably get a heating effect due to the "friction" of the ions among themselves. Fig. 9 illustrates a plan for carefully studying the effects of alternating currents upon electrolytes of different composition. The wiring and apparatus is so arranged in this experimental study as to allow of supplying alternating currents of the same energy value, but of various frequencies. It has been shown by the writer with such an experimental apparatus that the frequency of the alternations, everything else remaining the same, has a decided effect upon electrolytes. Only a very few years ago little had been done with the alternating current as applied to electrolytes, and nothing involving alternating currents with change of frequency. By "frequency" we mean the number of double reversals of the current per second. The frequency varies in practice between 25 and 150. The term "period" used in connection with an alternator, denotes the time elapsing between one complete reversal of the current. Now if we have free ions in solution which carry the electric current, they must move back and forth to some extent under the influence of an alternating current, in other words they must oscillate. Now by varying the frequency of our alternations we can vary the rate of oscillation of the ions, and if the heating is due to friction between the ions, the heating should be greater at higher frequencies than with low frequencies. Such was found to be the case, the energy value of the alternating current being kept the same. By a glance at the last illustration we can readily see how the frequency may be changed without altering the energy value of the current. We can strongly excite the fields of the alternator by admitting a heavy current through the rheostat, and drive the alternator by means of the motor at low speed, when we will obtain an alternating current of low frequency, and of a definite energy value. We can experiment with this arrangement. We can now turn in our rheostat, and admit a feeble current to the fields of the alternator, and by driving the armature at a high speed we will be able to obtain the same energy value for the current, but at high frequency. Experiments were also conducted with electrolytes of various compositions, that is with light and heavy ions present. The electrolytes consisting of light ions invariably heated up quicker than electrolytes with heavy ions. This can only be explained on the ground of inertia. The lighter ions travel through the greater distances when oscillating, and therefore collide a greater number of times. The heavier ions, because of their greater inertia, do not respond so readily to the alternations, and therefore move through a lesser distance. They consequently do not meet with so many collisions, and the friction is reduced. This of course is theory, but the fact of experimental investigation remains that the lighter ions cause a more rapid heating than heavy ions, and that all electrolytes heat up more quickly with alternating currents of high frequency than they do with alternating currents of low frequency. It only remains for us to find a theory to account for the facts.

(To be continued.)

BREEDING AND HEREDITY.*

WILLIAM BATESON, M.A., F.R.S.

HEREDITY—and variation too—are matters of which no naturalist likes to admit himself entirely careless. Everyone knows that, somewhere hidden among the phenomena denoted by these terms, there must be principles which, in ways untraced, are ordering the destinies of living things. Experiments in heredity have thus, as I am told, a universal fascination. All are willing to offer an outward deference to these studies. The limits of that homage, however, are soon reached, and, though all profess interest, few are impelled to make even the moderate mental effort needed to apprehend what has been already done. It is understood that heredity is an important mystery, and variation another mystery. The naturalist, the breeder, the horticulturist, the sociologist, man of science and man of practice alike, has daily occasion to make and to act on assumptions as to heredity and variation, but many seem well content that such phenomena should remain forever mysterious.

The position of these studies is unique. At once fashionable and neglected, nominally the central common ground of botany and zoology, of morphology and physiology, belonging specially to neither, this area is thinly tenanted. Now, since few have leisure for topics with which they cannot suppose themselves concerned, I am aware that, when I ask you in your familiar habitations to listen to tales of a no man's land, I must forego many of those supports by which a speaker may maintain his hold on the intellectual sympathy of an audience.

Those whose pursuits have led them far from their companions cannot be exempt from that differentiation which is the fate of isolated groups. The stock of common knowledge and common ideas grows smaller until the difficulty of intercommunication becomes extreme. Not only has our point of view changed, but our materials are unfamiliar, our methods of inquiry new, and even the results attained accord little with the common expectations of the day. In the progress of sciences we are used to be led from the known to the unknown, from the half-perceived to the proven, the expectation of one year becoming the certainty of the next. It will aid appreciation of the change coming over evolutionary science if it be realized that the new knowledge of heredity and variation rather replaces than extends current ideas on those subjects.

Convention requires that a president should declare all well in his science; but I cannot think it a symptom indicative of much health in our body that the task of assimilating the new knowledge has proved so difficult. An eminent foreign professor lately told me that he believed there were not half a dozen in his country conversant with what may be called Mendelism, though he added hopefully, "I find these things interest my students more than my colleagues." A professed biologist cannot afford to ignore a new life-history, the Okapi, or the other last new version of the old story; but phenomena which put new interpretations on the whole, facts witnessed continually by all who are working in these fields, he may conveniently disregard as matters of opinion. Had a discovery comparable in magnitude with that of Mendel been announced in physics or in chemistry, it would at once have been repeated and extended in every great scientific school throughout the world. We could come to a British Association audience to discuss the details of our subject—the polymorphism of extracted types, the physiological meaning of segregation, its applicability to the case of sex, the nature of non-segregable characters, and like problems with which we are now dealing—sure of finding sound and helpful criticism; nor would it be necessary on each occasion to begin with a popular presentation of the rudiments. This state of things in a progressive science has arisen, as I think, from a loss of touch with the main line of inquiry. The successes of descriptive zoology are so palpable and so attractive, that, not unnaturally, these which are the means of progress have been mistaken for the end. But now that the survey of terrestrial types by existing methods is happily approaching completion, we may hope that our science will return to its proper task, the detection of the fundamental nature of living things. I say return, because, in spite of that perfecting of the instruments of research characteristic of our time, and an extension of the area of scrutiny, the last generation was nearer the main quest. No one can study the history of biology without perceiving that in some essential respects the spirit of the naturalists of fifty years ago was truer in aim, and that their methods of inquiry were more direct and more fertile—so far, at least, as the problem of evolution is concerned—than those which have replaced them.

If we study the researches begun by Kölreuter and continued with great vigor until the middle of the sixties, we cannot fail to see that had the experiments he and his successors undertook been continued on the same lines, we should by now have advanced far into the unknown. More than this; if a knowledge of what those men actually accomplished had not passed away from the memory of our generation, we should now be able to appeal to an informed public mind, having some practical acquaintance with the phenomena, and possessing sufficient experience of these matters to recognize absurdity in statement and deduction, ready to provide that healthy atmosphere of instructed criticism most friendly to the growth of truth.

Elsewhere I have noted the paradox that the appearance of the work of Darwin, which crowns the great period in the study of the phenomena of species, was the signal for a general halt. The "Origin of Species," the treatise which for the first time brought the problem of species fairly within the range of human intelligence, so influenced the course of scientific thought that the study of this particular phenomenon—specific difference—almost entirely ceased. That this was largely due to the simultaneous opening up of lines of research in many other directions may be granted; but in greater measure, I believe, it is to be ascribed to the substitution of a conception of species which, with all the elements of truth it contains, is yet barren and unnatural. It is not wonderful that those who held that specific difference must be a phenomenon of slowest accumulation, proceeding by steps needing generations for their perception, should turn their attention to subjects deemed more amenable to human enterprise.

The indiscriminate confounding of all divergences from type into one heterogeneous heap under the name "Variation" effectually concealed those features of order which the phenomena severally present, creating an enduring obstacle to the progress of evolutionary science. Specific normality and distinctness being regarded as an accidental product of exigency, it was thought safe to treat departures from such normality as comparable differences; all were "variations" alike. Let us illustrate the consequences. Princess of Wales is a large modern violet, single, with stalks a foot long or more. Marie Louise is another, with large double flowers, pale color, short stalks, peculiar scent, leaf, etc. We call these "varieties," and we speak of the various fixed differences between these two and between wild *odorata*, as due to variation; and again, the transient differences between the same *odorata* in poor, dry soil, or in a rich hedge bank, we call variation, using but the one term for differences, quantitative or qualitative, permanent or transitory, in size, number of parts, chemistry, and the rest. We might as well use one term to denote the differences between a bar of silver, a stick of lunar caustic, a shilling, or a teaspoon. No wonder that the ignorant tell us they can find no order in variation.

This prodigious confusion, which has spread obscurity over every part of these inquiries, is traceable to the original misconception of the nature of specific difference, as a thing imposed and not inherent. From this, at least, the earlier experimenters were free; and the undertakings of Gärtner and his contemporaries were informed by the true conception that the properties and behavior of species were themselves specific. Free from the later fancy that but for selection the forms of animals and plants would be continuous and indeterminate, they recognized the definiteness of species and variety, and boldly set themselves to work out case by case the manifestations and consequences of that definiteness.

Over this work of minute and largely experimental analysis, rapidly growing, the new doctrine that organisms are mere conglomerates of adaptative devices descended like a numbing spell. By an easy confusion of thought, faith in the physiological definiteness of species and variety passed under the common ban

* Read before the section of zoology of the British Association for the Advancement of Science.

which had at last exorcised the demon Immutability. Henceforth no naturalist must hold communion with either, on pain of condemnation as an apostate, a danger to the dynasty of Selection. From this oppression we in England, at least, are scarcely beginning to emerge. Bentham's "Flora," teaching very positively that the primrose, the cowslip, and the oxlip are impermanent varieties of one species, is in the hand of every beginner, while the British Museum Reading Room finds it unnecessary to procure Gärtner's "Bastarderzeugung."

And so this mass of specific learning has passed out of account. The evidence of the collector, the horticulturist, the breeder, the fancier, has been treated with neglect, and sometimes, I fear, with contempt. That wide field whence Darwin drew his wonderful store of facts has been some forty years untouched. Speak to professional zoologists of any breeder's matter, and how many will not intimate to you politely that fanciers are unscientific persons, and their concerns beneath notice? For the concrete in evolution we are offered the abstract. Our philosophers debate with great fluency whether between imaginary races sterility could grow up by an imaginary Selection; whether Selection working upon hypothetical materials could produce sexual differentiation; how under a system of Natural Selection bodily symmetry may have been impressed on formless protoplasm—that monstrous figment of the mind, fit starting point for such discussions. But by a physiological irony enthusiasm for these topics is sometimes fully correlated with indifference even to the classical illustrations; and for many whose minds are attracted by the abstract problem of inter-racial sterility there are few who can name for certain ten cases in which it has been already observed.

And yet in the natural world, in the collecting box, the seed bed, the poultry yard, the places where variation, heredity, selection may be seen in operation and their properties tested, answers to these questions meet us at every turn—fragmentary answers, it is true, but each direct to the point. For if anyone will stoop to examine Nature in those humble places, will do a few days' weeding, prick out some rows of cabbages, feed up a few score of any variable larva, he will not wait long before he learns the truth about variation. If he go further and breed two or three generations of almost any controllable form, he will obtain immediately facts as to the course of heredity which obviate the need for much laborious imagining. If strictly trained, with faith in the omnipotence of selection, he will not proceed far before he encounters disquieting facts. Upon whatever character the attention be fixed, whether size, number, form of the whole or of the parts, proportion, distribution of differentiation, sexual characters, fertility, precocity or lateness, color, susceptibility to cold or to disease—in short, all the kinds of characters which we think of as best exemplifying specific difference—we are certain to find illustrations of the occurrence of departures from normality, presenting exactly the same definiteness elsewhere characteristic of normality itself. Again and again the circumstances of their occurrence render it impossible to suppose that these striking differences are the product of continued selection, or, indeed, that they represent the results of a gradual transformation of any kind. Whenever by any collocation of favoring circumstances such definite novelties possess a superior viability, supplanting their "normal" relatives, it is obvious that new types will be created.

The earliest statement of this simple inference is, I believe, that of Marchant,* who in 1719, commenting on certain plants of mercurialis with laciniated and hair-like leaves, which for a time established themselves in his garden, suggested that species may arise in like manner. Though the same conclusion has appeared inevitable to many, including authorities of very diverse experience, such as Huxley, Virchow, F. Galton, it has been strenuously resisted by the bulk of scientific opinion, especially in England. Lately, however, the belief in Mutation, as De Vries has taught us to call it, has made notable progress,† owing to the publication of his splendid collection of observations and experiments, which must surely carry conviction of the reality and abundance of Mutation to the minds of all whose judgments can be affected by evidence.

That the dread test of Natural Selection must be passed by every aspirant to existence, however brief, is a truism which needs no special proof. Those who find satisfaction in demonstrations of the obvious may amply indulge themselves by starting various sorts of some annual, say French poppy, in a garden, letting them run to seed, and noticing in a few years how many of the finer sorts are represented; or by sowing an equal number of seeds taken from several varieties of carnation, lettuce, or auricula, and seeing in what proportions the fine kinds survive in competition with the common.

Selection is a true phenomenon; but its function is to select, not to create. Many a white-edged poppy may have germinated and perished before Mr. Wilks saved the individual which in a few generations gave rise to the Shirleys. Many a black *Amphidasys betularia* may have emerged before, some sixty years ago, in the urban conditions of Manchester the black var. *doubledayaria* found its chance, soon practically superseding the type in its place of origin, extending itself over England, and reappearing even in Belgium and Germany.

Darwin gave us sound teaching when he compared man's selective operations with those of Nature. Yet how many who are ready to expound Nature's methods have been at the pains to see how man really proceeds? To the domesticated form our fashions are what environmental exigency is to the wild. For years the conventional Chinese primrose threw sporadic plants of the loose-growing *stellata* variety, promptly extirpated because repugnant to mid-Victorian primness. But when taste, as we say, revived, the graceful Star Primula was saved by Messrs. Sutton, and a stock raised which is now of the highest fashion. I dare assert that few botanists meeting *P. stellata* in Nature would hesitate to declare it a good species. This and the Shirleys precisely illustrate the procedure of the raiser of novelties. His operations start from a definite beginning. As in the case of *P. stellata*, he may notice a mutational form thrown off perfect from the start, or, as in the Shirleys, what catches his attention may be the first indication of that flaw which if allowed to extend will split the type into a host of new varieties each with its own peculiarities and physiological constitution.

Let anyone who doubts this try what he can do by selection without such a definite beginning. Let him try from a pure strain of black and white rats to raise a white one by breeding from the whitest, or a black one by choosing the blackest. Let him try to raise a dwarf ("Cupid") sweet pea from a tall race by choosing the shortest, or a crested fowl by choosing the birds with most feather on their heads. To formulate such suggestions is to expose their foolishness.

The creature is beheld to be very good after, not before its creation. Our domesticated races are sometimes represented as so many incarnations of the breeder's prophetic fancy. But except in recombinations of pre-existing characters—now a comprehensible process—and in such intensifications and such finishing touches as involve variations which analogy makes probable, the part played by prophecy is small. Variation leads; the breeder follows. The breeder's method is to notice a desirable novelty, and to work up a stock of it, picking up other novelties in his course—for these genetic disturbances often spread—and we may rest assured the method of Nature is not very different.

The popular belief that evolution, whether natural or artificial, is effected by mass-selection of impalpable differences arises from many errors which are all phases of one—imperfect analysis—though the source of the error differs with the circumstances of its exponent. When the scientific advocate professes that he has statistical proofs of the continuity of variation, he is usually availing himself of that comprehensive use of the term Variation to which I have referred. Statistical indications of such continuity are commonly derived from the study, not of nascent varieties, but of the fluctuations to which all normal populations are subject. Truly varying material needs care in its collection, and if found is often sporadic or in some other way unsuitable for statistical treatment. Sometimes it happens that the two phenomena are studied together in inextricable entanglement, and the resulting impression is a blur.

But when a practical man, describing his own experience, declares that the creation of his new breed has been a very long affair, the man of science, feeling that he has found a favorable witness, puts forward this testimony as conclusive. But on cross-examination it appears that the immense period deposed to seldom goes back beyond the time of the witness's grandfather, covering, say, seventy years; more often ten, or eight, or even five years will be found to have accomplished most of the business. Next, in this period—which, if we take it at seventy years, is a mere point of time compared with the epochs of which the selectionist discourses—a momentous transformation has often been effected, not in one character but many. Good characters have been added, it may be, of form, fertility, precocity, color, and other physiological attributes, undesirable qualities have been eliminated, and all sorts of defects "rogued" out. On analysis these operations can be proved to depend on a dozen discontinuities. Be it, moreover, remembered that within this period, besides producing his mutational character and combining it with other characters (or it may be groups of characters), the breeder has been working up a stock, reproducing in quantity that quality which first caught his attention, thus converting, if you will, a phenomenon of individuals into a phenomenon of a mass, to the future mystification of the careless.

Operating among such phenomena the gross statistical method is a misleading instrument; and, applied to these intricate discriminations, the imposing Correlation Table into which the biometrical Procrustes fits his arrays of unanalyzed data is still no substitute for the common sieve of a trained judgment. For nothing but minute analysis of the facts by an observer thoroughly conversant with the particular plant or animal, its habits and properties, checked by the test of crucial experiment, can disentangle the truth.

To prove the reality of Selection as a factor in evolution is, as I have said, a work of supererogation. With more profit may experiments be employed in defining the limits of what Selection can accomplish. For whenever we can advance no further by Selection, we strike that hard outline fixed by the natural properties of organisms. We come upon these limits in various unexpected places, and to the naturalist ignorant of breeding nothing can be more surprising or instructive.

Whatever be the mode of origin of new types, no theoretical evolutionist doubts that Selection will enable him to fix his character when obtained. Let him put his faith into practice. Let him set about breeding canaries to win in the class for Clear Yellow Norwich at the Crystal Palace Show. Being a selectionist, his plan will be to pick up winning yellow cocks and hens at shows and breed them together. The results will be disappointing. Not getting what he wants, he may buy still better clear yellows and work them in, and so on until his funds are exhausted, but he will pretty certainly breed no winner, be he never so skillful. For no selection of winning yellows will make them into a breed. They must be formed afresh by various combinations of colors appropriately crossed and worked up. Though breeders differ as to the system of combinations to be followed, all would agree that selection of birds representing the winning type was a sure way to fail. The same is true for nearly all canary colors except in lizards, and, I believe, for some pigeon and poultry colors also.

Let this scientific fancier now go to the Palace Poultry Show and buy the winning Brown Leghorn cock and hen, breed from them, and send up the result of such a mating year after year. His chance of a winner is not quite, but almost, *nil*. For in its wisdom the fancy has chosen one type for the cock and another for the hen. They belong to distinct strains. The hen corresponding to the winning cock is too bright, and the cock corresponding to the winning hen is too dull for the judge's taste. The same is the case in nearly every breed where the sex colors differ markedly. Rarely winners of both sexes have come in one strain—a phenomenon I cannot now discuss—but the contrary is the rule. Does anyone suppose that this system of "double mating" would be followed, with all the cost and trouble it involves, if Selection could compress the two strains into one? Yet current theory makes demands on Selection to which this is nothing.

The tyro has confidence in the power of Selection to fix type, but he never stops to consider what fixation precisely means. Yet a simple experiment will tell him. He may go to a great show and claim the best pair of Andalusian fowls for any number of guineas. When he breeds from them he finds, to his disgust, that only about half their chickens, or slightly more, come blue at all, the rest being blacks or splashed whites. Indignantly, perhaps, he will complain to the vendor that he has been supplied with no selected breed, but worthless mongrels. In reply he may learn that beyond a doubt his birds come from blues only in the direct line for an indefinite number of generations, and that to throw blacks and splashed whites is the inalienable property of blue Andalusians. But now let him breed from his "wasters," and he will find that the extracted blacks are pure and give blacks only, that the splashed whites similarly give only whites or splashed whites—but if the two sorts of "wasters" are crossed together blues only will result. Selection will never make the blues breed true; nor can this ever come to pass unless a blue be found the germ-cells of which are bearers of the blue character—which may or may not be possible. If the selectionist reflect on this experience he will be led straight to the center of our problem. There will fall, as it were, scales from his eyes, and in a flash he will see the true meaning of fixation of type, variability, and mutation, vaporous mysteries no more.

Owing to the unhappy subdivisions of our studies, such phenomena as these—constant companions of the breeder—come seldom within the purview of modern science, which, forced for a moment to contemplate them, expresses astonishment and relapses into indolent skepticism. It is in the hope that a little may be done to draw research back into these forgotten paths that I avail myself of this great opportunity of speaking to my colleagues with somewhat wider range of topic than is possible within the limits of a scientific paper. For I am convinced that the investigation of heredity by experimental methods offers the sole chance of progress with the fundamental problems of evolution.

In saying this I mean no disrespect to that study of the physiology of reproduction by histological means, which, largely through the stimulus of Weismann's speculations, has of late made such extraordinary advances. It needs no penetration to see that, by an exact knowledge of the processes of maturation and fertilization, a vigorous stock is being reared, upon which some day the experience of the breeder will be firmly grafted, to our mutual profit. We, who are engaged in experimental breeding, are watching with keenest interest the researches of Strasburger, Boveri, Wilson, Farmer, and their many fellow-workers and associates in this difficult field, sure that in the near future we shall be operating in common. We know already that the experience of the breeder is in no way opposed to the facts of the histologist; but the point at which we shall unite will be found when it is possible to trace in the maturing germ an indication of some character afterward recognizable in the resulting organism. Until then, in order to pursue directly the course of heredity and variation, it is evident that we must fall back on those tangible manifestations which are to be studied only by field observation and experimental breeding.

The breeding-pen is to us what the test tube is to

* Marchant, Mém. Ac. Roy. des Sci. for 1719; 1721, p. 59, Pls. 6 7. I owe this reference to Coutagne, "L'hérédité chez les vers à soie." (Bull. sci. Fr. Belg. 1902.)

† This progress threatens to be rapid indeed. Since these lines were written Prof. Hubrecht, in an admirable exposition (Pop. Sci. Monthly, July, 1904) of De Vries' "Mutations-theorie," has even blamed me for having ten years ago attached any importance to continuous variation. Nevertheless, when the unit of segregation is small, something mistakably like continuous evolution must surely exist. (Cp. Johannsen, "Ueb. Erblichkeit in Populationen und in reinen Linien," 1903.)

the chemist—an instrument whereby we examine the nature of our organisms and determine empirically what for brevity I may call their genetic properties. As unorganized substances have their definite properties, so have the several species and varieties which form the materials of our experiments. Every attempt to determine these definite properties contributes immediately to the solution of that problem of problems, the physical constitution of a living organism. In those morphological studies which I suppose most of us have in our time pursued, we sought inspiration from the belief that in the examination of present normalities we were tracing the past, the phylogenetic order of our types, the history—as we conceived—of Evolution. In the work which I am now pressing upon your notice we may claim to be dealing not only with the present and the past, but with the future also.

On such an occasion as this it is impossible to present to you in detail the experiments—some exceedingly complex—already made in response to this newer inspiration. I must speak of results, not of methods. At a later meeting, moreover, there will be opportunities of exhibiting practically to those interested some of the more palpable illustrations. It is also impossible to-day to make use of the symbolic demonstrations by which the lines of analysis must be represented. The time cannot be far distant when ordinary Mendelian formulæ will be mere *as in praesenti* to a biological audience. Nearly five years have passed since this extraordinary rediscovery was made known to the scientific world by the practically simultaneous papers of De Vries, Correns, and Tschermak, not to speak of thirty-five years of neglect endured before. Yet a phenomenon comparable in significance with any that biological science has revealed remains the intellectual possession of specialists. We still speak sometimes of Mendel's hypothesis or theory, but in truth the terms have no strict application. It is no theory that water is made up of hydrogen and oxygen, though we cannot watch the atoms unite, and it is no theory that the blue Andalusian fowl I produce was made by the meeting of germ-cells bearing respectively black and a peculiar white. Both are incontrovertible facts deduced from observation. The two facts have this in common also, that their perception gives us a glimpse into that hidden order out of which the seeming disorder of our world is built. If I refer to Mendelian "theory," therefore, in the words with which Bacon introduced his Great Instauration, "I entreat men to believe that it is not an opinion to be held, but a work to be done; and to be well assured that I am laboring to lay the foundation, not of any sect or doctrine, but of human utility and power."

In the Mendelian method of experiment the one essential is that the posterity of each individual should be traced separately. If individuals from necessity are treated collectively, it must be proved that their composition is identical. In direct contradiction to the methods of current statistics, Mendel saw by sure penetration that masses must be avoided. Obvious as this necessity seems when one is told, no previous observer had thought of it, whereby the discovery was missed. As Mendel immediately proved in the case of peas, and as we have now seen in many other plants and animals, it is often impossible to distinguish by inspection individuals whose genetic properties are totally distinct. Breeding gives the only test.

(To be continued.)

A NEW PROCESS OF TESTING LUBRICATING OILS.*

By EMILE GUARINI.

IN the testing of lubricating materials for engines, two points are to be considered—the degree of fluidity of the oil and the internal resistance of friction. It

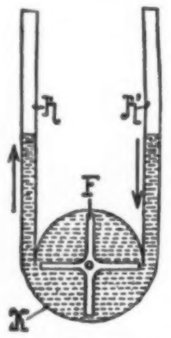


DIAGRAM OF AN APPARATUS FOR TESTING LUBRICATING OIL.

is for the determination of these two factors that the arrangements recently devised by the Allgemeine Elektricitätsgesellschaft of Berlin is designed. In this process, there is employed as a measurement of internal resistance the displacement of a column of fluid at the base of which some particles of the oil to be studied are set in motion. The greater the internal friction of the oil to be tested, the greater also will be the effect upon the column. The various displacements of the latter, therefore, constitute a simple and easily comparable measurement for the different qualities of oils.

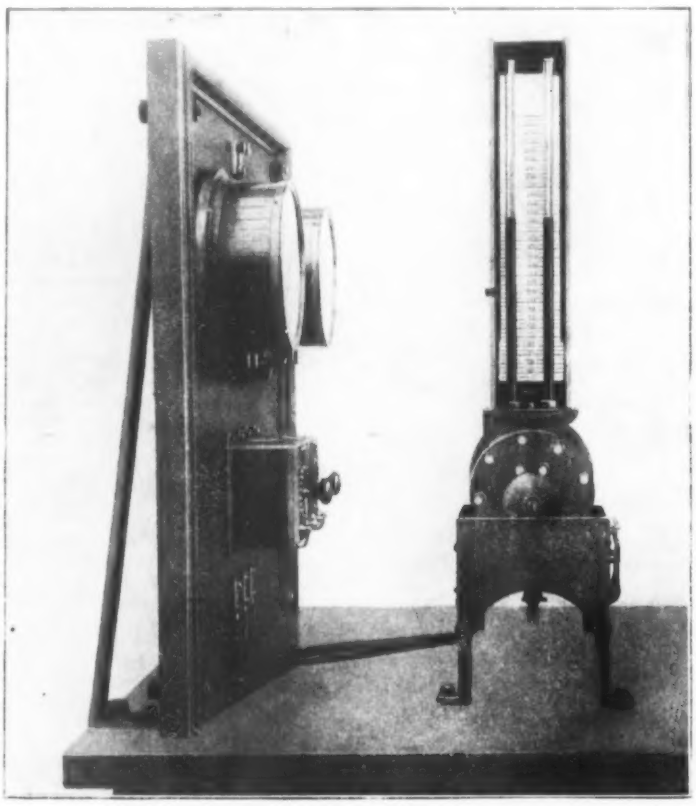
The apparatus consists essentially of a closed chamber filled with the oil to be tested and in the interior of which revolves a paddlewheel actuated by an electric motor. This chamber communicates with two graduated columns placed diametrically opposite each other. These tubes, as well as the chamber, are filled with the oil to be tested, but only for half of their height.

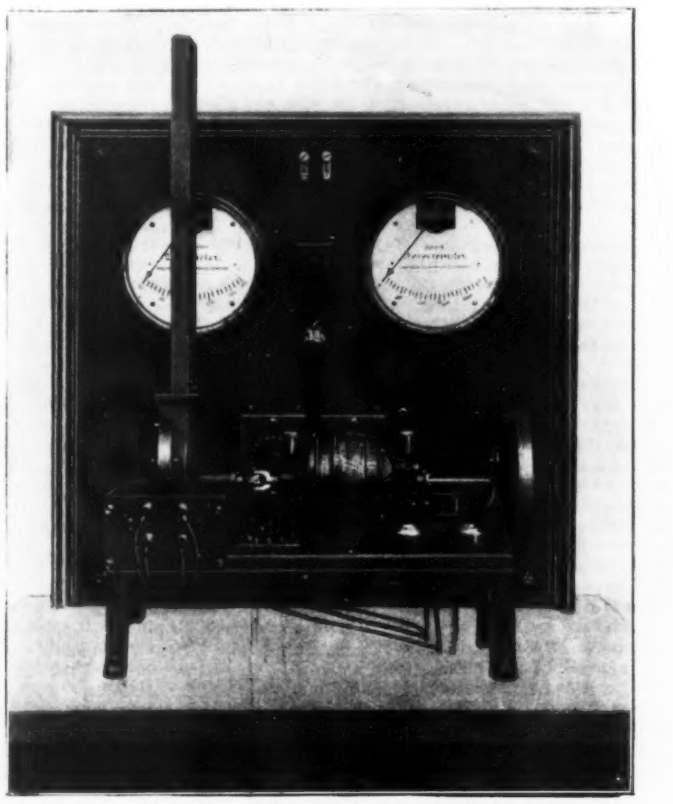
When the paddlewheel begins to revolve, the oil is violently stirred, and friction is thus produced at the base of the tubes. The frictional resistance that occurs causes the liquid to ascend in one of the columns and descend in the other. The difference in height of the two columns of liquid makes known, through the intermedium of the specific weight and the temperature of the liquid, the degree of the internal frictional resistance of the lubricating oil.

In order to render it possible to test the oil at various temperatures, the arrangement is provided with an electric heating apparatus designed to heat the oil chamber. On the other hand, a rheostat permits of greatly varying the number of revolutions of the motor. Finally, the complete arrangement includes a reversible electric revolution counter, a voltmeter, and an ammeter. The whole is mounted upon a small table provided with a distributing board.

In order to obtain definitive results, the difference of height between the two columns of liquid is ascertained, and this is multiplied by the specific weight of the oil at the temperature at which the test is made. Then the peripheric velocity is calculated from the number of revolutions and the diameter of the paddlewheel. The operation is performed in this way for the entire series of observations at different temperatures. If, through any cause whatever, especially a change of temperature, the speed of displacement of the oil is modified, the desired corrections will have to be introduced into the difference of the heights of the columns of liquid. In this way, very useful curves are established. For the observations that serve for determining the friction resistance of the oil independently of the speed, the lower points of difference of height that correspond to the internal difference of friction are employed as ordinates and the corresponding peripheric velocities as abscisses. The lower the points are for equal velocities, the flatter will become the curve and less will be the internal friction of the oil tested. If it be desired to render the comparative figures independent of the different trial apparatus, it will be necessary to express in per cent the values of the height of elevation reduced to the unit of specific weight, and that, too, according to the values that have been obtained with one and the same apparatus and at equal velocities with a normal oil. As a normal oil, petroleum may be used to advantage, since its consistency is but slightly affected by the temperature. The actual degree of consistency is obtained from the series of observations made with a constant velocity and a temperature that increases according to the power developed by the engine.

As a usual thing, there is no occasion for introducing corrections for the various heights of the column of liquid or of the various specific weights. The values obtained may also be put in the form of diagrams, the power being taken as an ordinate and the temperature as an abscissa. Here also a comparison may be advantageously made with petroleum.





AN INSTRUMENT FOR TESTING LUBRICATING OILS BY MEANS OF THEIR INTERNAL ELECTRICAL RESISTANCE.

Another curve, unimportant for the characteristics of the oil, is obtained by means of the values of the series of observations made with a constant velocity and a gradually rising temperature, when independently of the temperature, we inscribe the variations of the resistance of internal friction represented by the differences in height of the columns of liquid after a reduction of the specific weight to unity. Upon fixing a maximum value for the internal resistance of friction, we obtain from these curves the temperature at which it is possible to most profitably use the oil tested in this manner. Besides, through the greater or less diminution of the resistance of internal friction of the oil as a function of the increase in temperature, it is possible to see how well the oil is adapted for specific high pressures in the pillow-blocks. High pressures at the bearings require, for greater safety in the running of an engine, a notable diminution of the internal friction resistance with the gradual elevations of temperature. In order to better compare different kinds of oils, it is important to bring together the various data of the experiments in the form of tables.

The arrangement under consideration seems to be destined to render great services, especially in central electric stations and electrically-driven works, as well as in large manufactories in which steam is employed, and in which, in consequence of the large quantity of it used, the quality of lubricating oil is of considerable economic importance.

* Specially prepared for the SCIENTIFIC AMERICAN SUPPLEMENT.

Waterproofing Composition for Boots.—Fuse together, taking precautions against the mass catching

fire: Lard, 10 pounds; sheep tallow, 20 pounds; oil of turpentine, 5 pounds; yellow wax, 5 pounds; olive oil (sec), 6 pounds. Rub well into the warm boots about 48 hours before they are to be worn.—Wiener Seifensieder Zeitung.

A FRENCH OPINION ON AMERICAN LOCOMOTIVES.*

By DANIEL BELLET, Professor at the Paris Ecole des Sciences Politiques.

It will be extremely interesting at a moment when certain foreign countries—notably England—are instituting tests of French locomotives of the De Glehn type, such as are possessed by the great French railway systems, to follow the experiments which have been made in France with the American machines, and thus make the same comparison from the opposite point of view.

Our readers will no doubt be aware that two of the largest French companies possess engines of American construction; one, the State Railway (Chemin de Fer de l'Etat) which, as its name indicates, is run by the State itself, and the other the Paris-Lyons-Mediterranean Company (the P. L. M.). The latter company has only bought one American machine, and that not very long ago, so that the experiments made up to the present are considered inconclusive, especially as M. Chabal—chief superintendent of locomotives and stock to the company—who always welcomes us most cordially, has just informed us that it is as yet impossible for him to express a decided opinion as to the comparative merits of the American and French machines, or as to the particular advantages or disadvantages of the former.

The State Railway Company has followed these experiments for a much longer period; the state, indeed, claims that it gives lessons in progress to the private companies, and wished to establish whether or no the American pattern might give better results than the French. With this object in view, the state has acquired successively American engines of two different types, which we must describe rather precisely, in order that the practical results they have given in drawing regular trains may be thoroughly appreciated.

The former of these types is a Vauclain compound, that is to say, with two superposed cylinders, one high and the other low pressure, the rods of the two pistons commanding a single crosshead. The distribution of steam is controlled by a double piston valve working in a distributor cylinder placed on the side toward the interior.

The principal characteristics of this type of machine are: 84-inch diameter driving wheels; 23 feet 6 inches between the extreme axles; the cylinders have the respective diameters of 13 inches and 22 inches for a stroke of 26 inches. The area of the grate is 25.68 square feet, that of the furnace 129 square feet; area of the tubes is 1,571 square feet; therefore the total area is 1,700 square feet. The indicated pressure of the boiler is 218 pounds per square inch. We may add that the weights, in working order, of this class of engine are 48,400 pounds for the bogie, 36,520 for the third axle, and 35,640 for the fourth; that is, 120,560 altogether. It is hardly necessary to say that we are speaking of machines with two coupled axles.

The second American machine, which was minutely and methodically tried with a view to comparison with the French machines, was an engine of the Atlantic type. It is a simple expansion engine, and its characteristics are as follows: driving wheels of 84-inch diameter, like the last mentioned; there are two pairs of coupled driving wheels, a four-wheeled bogie in front and two trailing wheels. The distance between the extreme axles is 26 feet 7 inches; the diameter of the cylinders is 18 inches for a stroke of 26 inches. The boiler pressure is again 218 pounds. The grate area is 35 square feet, that of the fire-box 171½ feet; the area of the tubes is 1,720 feet, giving a total of 1,892 square feet. The respective weights are: 16 tons for the bogie, 17 on the third axle (which, as we have seen, is coupled), 17 also on the fourth, and only 15 on the trailer.

The comparison of these two locomotives with the French engines has been made by M. Nadal, general superintendent of locomotives and stock on the state railway, after methods more or less of his own, which methods, by the way, have been considerably criticised by engineers of the other French companies. He has submitted all the machines to exactly the same tests. The engines which were thus simultaneously studied belong to different classes, and the brief description of them which we are about to give has the advantage of showing what kind of machine the state employs.

First of all, they were the engines which were put into service in 1901 and 1902, and which are very similar to the greater number of the new machines possessed by the other great French systems. They have six coupled wheels of 69 inches diameter. The respective diameters of the cylinders are 12.8 and 21.7 inches for a stroke of 25.2 inches. The indicated boiler pressure is 218 pounds, the grate area is 25½ square feet, and as the area of the firebox is 129.9 square feet, and that of the tubes (which are of the Serve type) 1,697 square feet, the total heating area reaches 1,826.9 square feet.

Comparative trials have also been made of machines of a somewhat older type, simple expansion locomotives in fact, with a pressure of 188 pounds, with two leading wheels, then four coupled driving wheels, and two trailers. The diameter of the driving wheels is 79.6 inches.

Lastly, comparative tests were made with a simple expansion engine of 200 pounds, having four leading wheels and four driving wheels; the diameter of the latter is 80 inches, and it is fitted with the Ricour cylindrical slide valves.

M. Nadal considers that the French regulator, so to speak, with a double flat slide valve, is inferior to that of the American machines consisting of a double-seat balanced valve, as the latter presents greater working facility, and permits more easily the regulation of the openings; it is, however, less reliable, or rather, its adjustment is more difficult than is the case with those of the French machines.

The pressure of steam in the steam chest varies very little in all the machines experimented upon, excepting in the case of the French Glehn machine using 218 pounds boiler pressure. It is necessary to point out that in these engines, to which we alluded in speaking of the new machines put into service in 1901 and 1902, the space occupied by the steam between the regulator and the cylinders is only 3.88 cubic feet, that is to say, 3.4 times the capacity of an average cylinder; while in the other machines the volume represents from 4.4 to 5.4 times a cylinder. In the Vauclain machine, for instance, the capacity of one cylinder is 1.3 cubic feet, and that of the pipes and steam chest 6.02 cubic feet. For the American machine Atlantic the corresponding figures are: 1.17 and 5.7. For the locomotive which we have designated by the name Glehn, the figures are also 1.17 and only 3.88 for the pipes and steam chest.

Special abstracts have been made in order to obtain coefficients of the wire-drawing of the steam for these different machines, and in this respect the inferiority of the Glehn, and still more of the simple expansion engine of 14 kilos, to the American machines is very marked, as it is also to the simple engine.

The coefficient in question is, in fact, only 0.20 for the American Vauclain, while for the Glehn it is 0.33. We must, however, be just in other respects, and it is therefore necessary to note that the back pressure on the piston during its return is sensibly higher in the American engines, and on the French simple engine than in the Glehn; in the case of the last named the back pressure is lower by about one-third, which naturally gives it a certain superiority.

The tractive effort reaches its maximum at a certain speed between 50 and 75 miles an hour in the machines which have been studied. For the Vauclain it is 5,610 pounds for a speed of 62 miles as against 3,630 for the Glehn.

In the trials and comparisons of steam consumption per horse-hour, it has been possible to ascertain the influence of the regulation pressure and that of the degree of expansion. The lowest consumption is met with in the two compound engines, the Vauclain and the Glehn; it varies from about 15.4 to 17.6 pounds, according to the pressure. Next comes the improved valve-gear machine, the French simple engine, on which this consumption easily surpasses 17.6 pounds. The figure is still higher on the Ricour engine of 200 pounds pressure, and only the last place is taken by the Atlantic, on which the consumption springs to 20.7 and 21.3 pounds, while it never reaches 21.3 on the 200-pound machine, of which we have just spoken. It must be remarked that this high consumption for the Atlantic proceeds to a great extent from the escape of steam through the valve gear during the expansion. It is on account of these escapes that the French engineers find fault with the American machines. They complain of their imperfect adjustment, and it is certain that but for these escapes the steam consumption would be about the same as on the French 200-pound machines.

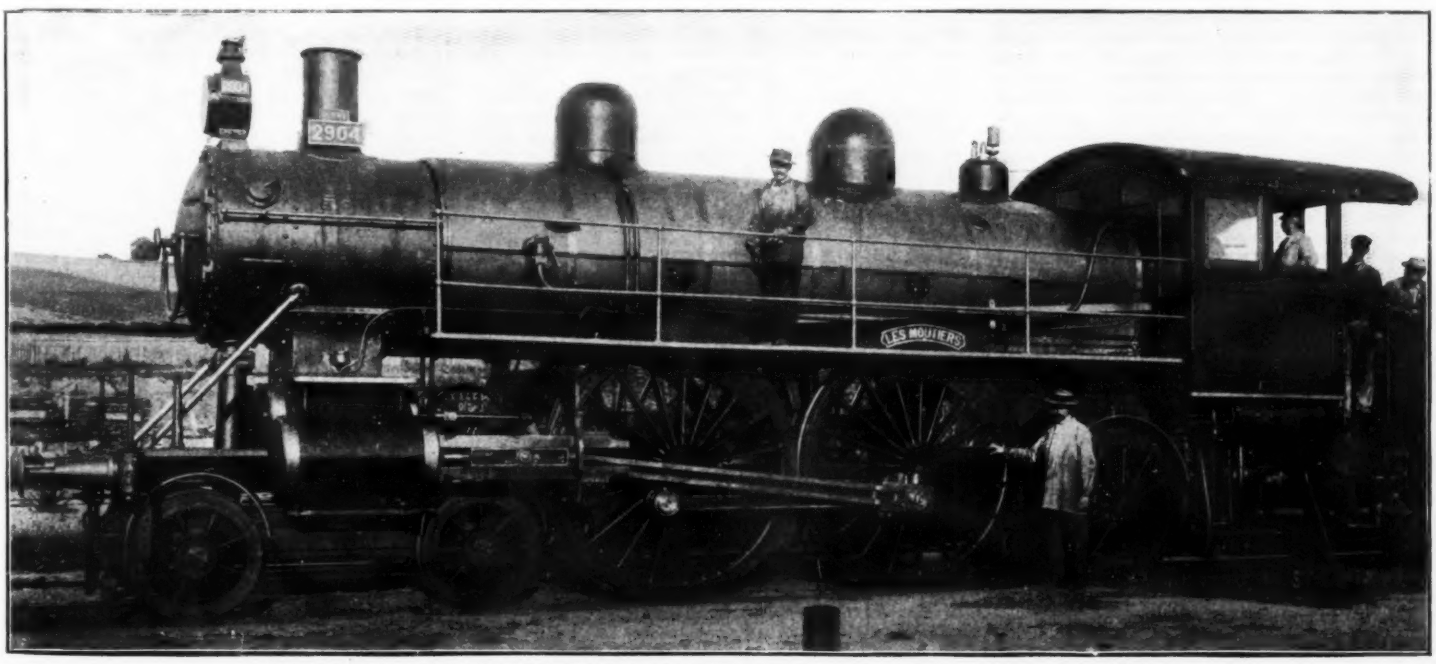
M. Nadal has calculated the quantity of steam condensed by each stroke of the piston, by means of a formula which we, however, shall not give here. It may be noted that in this respect the Vauclain seems to be a little superior to the Glehn, which latter calls for our special attention, as it is the most highly perfected type of French locomotive at present. With the former the quantity of steam condensed per horse-power-hour is from 43.56 pounds to 47.96, while with the latter the corresponding quantity varies from 54.78 to 61.38, which is considerably higher. For the Atlantic the figure oscillates between 67.76 and 72.6 pounds.

As to the actual comparative consumption of water per horse-power-hour in the different types of engine experimented upon: For the Vauclain machine this consumption is 26.64 pounds, and it attains 29.57 for the Atlantic, while it may be as low as 23.54 pounds for the Glehn. It is not necessary for us to remark that here is a very great difference, and one that is extremely advantageous to this class of engine. For the two other French machines the corresponding figures are 29.74 pounds for one (the machine at 200 pounds pressure) and 26.4 pounds for the second.

We shall complete this indication by a short reference to the rate of steam production, or in other words to the quantity of water vaporized per pound of fuel burned. Here a characteristic detail immediately becomes apparent; that is, that the rate of production is strikingly highest on the American Vauclain machine, being in fact 8 or even 8.09 pounds, while it reaches only 7.27 on the Glehn machine, and respectively 6.78 and 7.16 pounds on the other two French machines. On the other hand, we must point out that the rate of steam production is only 6.66 for the machine of the Atlantic type.

M. Nadal, however, considers that the Vauclain's high rate is accountable solely to the great proportion of water in the steam, and he estimates that for the Vauclain, as for the others, the effective production of dry steam would be about 7 pounds, and that the other pound is admixed water.

We have just seen that the compounds show a notable economy of dry steam per indicated horse-power, and M. Nadal thinks this is due, not only to the dimi-



AMERICAN VAUCLAIN LOCOMOTIVE OF THE FRENCH CHEMIN DE FER DE L'ÉTAT.

* Specially prepared for the SCIENTIFIC AMERICAN SUPPLEMENT.

nution of condensations, but also to a steam consumption apparently lower because the mean pressure is lower, and because the expansion takes longer than with the non-compounds.

In nearly all the European countries, and particularly on the French railway systems, the American machines are held to be less economical than those of Europe; and it will evidently be of interest, and at the same time useful, to show to what conclusions the French engineer has come upon this point. We may as well say at once that this reputation is considered to be perfectly justified, at all events was so when the machines were put into service, and before certain modifications and adjustments were made.

Does the blame rest with the boiler, the furnace, or the cylinders and valve gear?

In the same order of inquiry we should also consider the admixture of water with the steam as having a serious influence on the economic working of a locomotive.

According to M. Nadal, the output of the boiler is really inferior in the American machines to what it is in the French, and he attributes this fact principally to the disposition of the blast pipe, which is fixed and double; and, if the expenditure of water is often excessive on the American machine, it will be due to the small expanse of its surface, its level being very high in the boiler, and consequently the space available for the steam above this level is too small. Again, escapes and leakages are frequent in machines of American fabrication, notably at the crossbars of the tube plate, at the stays, at the foundation ring, and at the tubes; these escapes, however, mean nothing but that the materials used are not of the best quality. M. Nadal fears that this is due to the dearness of American craftsmanship, and he finds that the execution generally, even in the construction of the boiler, leaves much to be desired.

He is convinced that the steam in the cylinders may be utilized quite as well as in the European machines, but only when a sufficiently high pressure be maintained in the steam chest—a pressure of at least 125 or 150 pounds, with the degrees of admission at from 20 to 30 per cent. If the admission were at 40 to 50 per cent, it seems to M. Nadal that the steam expenditure would be too great, on account of the insufficient expansion.

As far as the balanced valves, flat or cylindrical, with which the American machines are fitted are concerned, M. Nadal is very favorable in theory. He considers that the balance is advantageous because of the diminution of friction, and that it is even necessary with the high pressures which obtain at present, and which are being more and more exclusively employed. But these valves are rarely reliable, and this reproach is particularly applicable to the Vauclain and Atlantic engines which were tried on the state lines of France.

The practical drawbacks are specially noticeable in the case of the cylindrical valves, which simply consist, like the motor pistons, of a piston of cast iron fitted with two rings. The openings of these rings rapidly widen, and if the piston rod be not adjusted exactly, there is a direct escape toward the release; again, the envelopes soon wear out at the edges of the ports, and the arrival of the steam is then no longer completely intercepted at the moment which, theoretically, ends the admission phase, but only during the expansion, sometimes even later. As concerns more particularly the Vauclain machine, the presence of a single distributor for the two cylinders—a distributor composed of four cylindrical valves, mounted on the same rod—renders the escapes still more frequent; the envelope is pierced with numerous apertures, and is extremely awkward to adjust so as to fit properly.

We may add that the defective adjustment, of which M. Nadal complains in the boiler, is to be met with more or less in the general construction of the American locomotives, which gives rise to a higher interior resistance and, in consequence, reduces the tractive power.

We find, in comparing the different machines of which we have had to speak, that the average resistance per ton of the machine and tender varies between very wide limits, even in doing the work necessary for hauling the same train.

In this respect, the various comparisons are well established; it has been found that where the Atlantic, weighing 92 tons, devotes 899,510 foot-pounds to the hauling of the train, the Glehn devotes 930,546 with its own superior weight of 98 tons. Under other conditions the respective figures of 509,101 and 566,228 were shown by the same two machines. If, on the other hand, we compare the results shown by the Vauclain and the Glehn machines, we find that the work devoted, as has been explained, to the haulage of trains is expressed for the former by the figure 419,178, while it is 381,706 for the other. (The respective weights being in this case 80 and 92 tons.) With the same train the work devoted to its haulage by a simple expansion machine at 200 pounds, was from 416,702 to 467,263. From these observations it has been possible to draw the conclusion that the compound machines—either the American Vauclain or the French Glehn—have to develop from 60 to 100 horse-power more than the others, on account of the work necessary to their own haulage. If we represent the value of the resistance of engine and tender by 12.20 per ton, and at a speed of 50 miles per hour, for the machine of the Bonnefond system, we find that the corresponding resistance for the machine at 200 pounds pressure with the Ricour valve gear is 10.13 and—which is practically the same—10.14 for the non-compound American engine, i. e., the Atlantic. That is the group of simple expansion machines. For the compound machines the figure is found to be 12.8 both for the American of the Vauclain type and for the French Glehn.

One cannot but be struck by this identity, and it will also be remarked that the resistance of the compound machines is very much higher than is the case with the others—at least, at a speed of 50 miles, at which the experiments were made.

It follows, perforce, that part of the economy effected in consumption per indicated horse-power by the compound is counterbalanced by the machine itself. With regard to this point, we may add that M. Nadal wished to compare these two types of machine, taking into account, first, the actual work done—the work at the drawbar of the tender; second, the corresponding expenditure. He has ascertained, for instance, that a compound engine of the two types we have under discussion insures an economy of 18.5 per cent over a machine of simple expansion, when the speed is 31 miles and the hauled weight 615 tons on a rising gradient of 5 in 1,000. For a load of 320 tons on the same gradient, and at a speed of 46.3 miles, the economy would not be more than 11.2 per cent; at a speed of 62.1 miles on the level, with a load of 400 tons there would still be a saving of 4.9 per cent; but if the load were only 144 tons, an inferiority of —1.5 per cent would be shown.

THE ACTION OF PUMPS.

From a hasty and superficial consideration, says the Practical Engineer, no mechanical operation appears simpler than pumping fluids, whereas, under some considerations, it requires most carefully-designed machinery, and a thorough grasp of all the effects of moving fluids, before the operation can be successfully accomplished. Referring in the first place to the suction action of a pump, the plunger is followed by a plug of water in the whole length of the suction piping, and the effect of this water may be likened to so much dead weight added to the plunger, the inertia of which will be felt by the reciprocating parts and the crank pin. This action will show that an air chamber on the suction is a very necessary provision where the length of suction pipe is considerable. Turning now to the delivery action, we have, when the pump is about to commence its stroke, the whole mass of water in the pump chamber and the rising main inert, and this mass is suddenly set in motion by the advance of the plunger. It is clear, therefore, that severe shocks will occur where the delivery pipe is of great length. The above conditions exist where the valve action of the pump is perfect, but when faulty valves are present further shocks will be encountered. Consider, for instance, the effect of a sluggish closing of the delivery valves. When the plunger has attained its extreme stroke and begins to return, it will be followed by the whole volume of water in the rising main, but this motion will only continue so long as the delivery valve remains open. When this has closed the motion will be suddenly arrested, and great shocks will be felt unless air chambers are provided. In addition to the actions just described, there is the effect of a sluggish action on the part of the suction valves, as well as the effect of a restricted suction passage, but enough has already been said to indicate the nature of the shocks encountered by pumps. When these are considered, the frequent fractures and breakdowns which occur will not appear to be very mysterious; and this article will have served its purpose if it will lead some designers to realize that pumps have to withstand other stresses than those due to the mere statical head of the fluid.

COMPRESSED AIR IN HOISTING.

It has long been known that compressed air forms a convenient and practical method of transmitting power to one or a number of hoisting engines, particularly where water or other cheap power is available. This method of transmitting power has until recently been of low efficiency and consequently wasteful of power, and has been attended with much difficulty from the refrigerative action of compressed air during expansion, causing the formation of frost and ice in the exhaust and cylinder of the engine. Recent improvements in compression and use of air have opened up a new, enlarged, and profitable field for compressed air, of which we will speak later.

Advantages of compressed air in hoisting are greatest in cases where one modern, central power air-compressing plant can be used to drive a number of hoists, large and small, located so far apart as to require one or more boilers at each hoisting station, involving a supply of fuel and water for each and the disposal of ashes. A number of such boilers scattered about a mine or quarry working intermittently, sometimes not called on for steam for hours and at other times overworked, are very wasteful of fuel and seldom give an average of efficiency of over 3 or 4 pounds of water evaporated per pound of coal. On the other hand, a first-class boiler plant at a central power station can be made to give efficiency of 8 or 10 pounds of water per pound of coal. A hoisting engine driven by air under these circumstances is ready for instant, continuous service at full power and speed, no time being lost in working out the water or in quickening the fire, in case it has become dull, and no loss of steam from the safety valve, in case of stoppage of the hoist with a hot fire under the boiler. Small boilers give trouble from freezing of connections at night, on Sundays, and holidays, and require banking of fires or keeping them up at a cost that is a large percentage of the actual running expenses of these particular hoists.

In mining and quarry work it often happens that demand for certain sizes or quality of product renders it desirable to lay up some hoists and perhaps work others beyond the capacity of the boiler. Air is much more flexible than steam and lends itself to help out in these emergencies. The points mentioned are not theoretical, but practical, and were brought forcibly to the writer's attention recently while inspecting the working of the central air plant installed at No. 6 Quarry of the Cleveland Stone Co. at North Amherst, Ohio. This, without doubt, is capable of producing the largest amount of air per pound of fuel of any steam plant in this country, and of a quality so dry that there has been no trouble from freezing through the past unusually cold winter, although there are several miles of pipe lines all in the open air, and some of the hoisting engines are located nearly a mile from the central station. It is a popular fallacy that compressed air freezes; in reality it is the water in the air that freezes, and, if the air is made dry, which is perfectly practicable, there will be no freezing in the pipes.

When this quarry was operated by a large number of steam boilers it would take about 30 minutes in the morning to get into full working shape with the hoists, channelers, and drills, and little work was done the last 15 to 30 minutes before quitting time, especially in the winter, owing to getting ready for leaving the boilers for the night. With the use of air, work begins on the blowing of the whistle, and is in full swing within 5 minutes, as indicated by the number of revolutions made by the compressors. It continues up to the blowing of the whistle at quitting time.—J. S. Lane in Mines and Minerals.

EXPERIMENTS WITH SUPERHEATED STEAM FOR LOCOMOTIVES.

The Prussian Railway authorities carried out experiments on the effect of superheating by fitting one of each of two pairs of locomotives with Pielock superheaters built round the tubes immediately under the dome. The results of these experiments were given by M. Strahl in Zeitschrift des Vereines deutscher Ingenieure. For comparison of the engines, with and without superheaters, they were run on the same journeys alternately, and under the same conditions as far as possible. No attempt was made to indicate the engines, but it was assumed that the same class of engines hauling equal weights of trains at the same speed and under similar conditions expended the same power, and on this basis a comparison was possible by ascertaining the quantity of water evaporated, knowing also the pressure and temperature of the steam. From these data the amount of heat taken up by the steam in the boiler and in the superheater can be calculated, and the comparison of the quantities of heat shows the relative economies of the engines with and without the superheater. Let λ and λ^1 denote the total heat of saturated and superheated steam respectively, and q_0 the heat of the feed water, G and G^1 the quantities of water evaporated, and Q and Q^1 the amounts of heat used with and without the superheater. In the experiments with the pair of high-pressure engines, the amounts of steam used were (1) with the superheater 3.66 tons, (2) without superheater 4.34 tons; showing an economy of 16 per cent of steam due to superheating. Further,

$$\frac{Q^1}{Q} = \frac{G^1 (\lambda^1 - q_0)}{G (\lambda - q_0)}$$

and in the case of the engine provided with superheater the pressure being 13 atmospheres (185 pounds per square inch), the steam temperature 260 deg. C. and the feed temperature 10 deg. C., the working out of the above formula shows that the ratio of Q^1 to Q is 88 : 100, which is equivalent to a saving of heat—and therefore coal—of 12 per cent due to superheating. The actually measured quantities of coal showed a saving of 12.3 per cent, and the correspondence of the observed with the calculated saving of coal proves that the efficiency of the boiler was not lessened by the addition of the superheater. It was found that the engines with superheaters used almost exactly the same volume of steam as those without superheaters. On the basis of his experimental results, the author calculates the economy to be expected with various degrees of superheat, the pressure being 13 kilogrammes per square centimeter (185 pounds per square inch). With 10 degrees of superheat the amounts of saving in water and coal are 2.5 per cent and 2 per cent respectively. With 40 degrees and 70 degrees of superheat (230 deg. C. and 260 deg. C. steam temperature) the amounts of saving in steam are 10 per cent and 16 per cent respectively, and 7 per cent and 12 per cent in coal; this corresponds with the actual experiments.

With regard to the effect of highly-heated steam on the lubrication, the highest temperature attained was 272 deg. C., and there was no difficulty with the ordinary slide valve, plenty of oil being supplied to the sliding surfaces by means of forced lubrication.—Proc. Inst. C. E.

An Excellent Mucilage for Affixing Paper Labels to Glass, etc., is obtained by pouring 300 grammes of boiling water on 50 grammes of chloral hydrate, 80 grammes of white gelatine, and 20 grammes of gum arabic in a porcelain vessel and setting aside with frequent stirring. If the glue is too thick, it is placed

before use in warm water. When it is desired to fasten paper labels on tin vessels, it is advisable to coat the places previously with diluted dammar or copal varnish (1 part of oil of turpentine and 2 parts of varnish).—Deutsche Drogisten Zeitung.

CO-OPERATION AMONG AMERICAN GEOGRAPHICAL SOCIETIES.*

By ISRAEL C. RUSSELL.

In considering the many ways in which the science having as its special province the study of the earth's surface can be enhanced and its service to mankind rendered more efficient through the agency of geographical societies, five subordinate themes present themselves for consideration. These are: The scope and aim of geography; the methods of gathering and distributing geographical knowledge; the functions of geographical societies; the present status of the geographical societies in America; and in what ways can the geographical societies of this country increase their influence and enlarge their usefulness.

Geography has been well defined recently as the science which deals with the distribution of every feature and the environment of every creature on the face of the earth. The geographer, however, must remember that the earth's surface is not fixed and rigid, a dead, motionless thing, but ever changing in response perhaps to the fall of a raindrop or an eruption of Krakatoa, and that it is clothed with beauty both of form and color, and whispers with a thousand tongues to the admirer who inclines a listening ear. What then is geography? The study of the distribution of earth features and of the environment of living things, to be sure, but also the reading of the fascinating story of the development of those features, and a search for the complex antecedent conditions which gave birth to the present marvelously delicate adjustment of life to its environment. Illuminating this temple not made by hands are pictures of the earth beautiful, and the many charms that are imparted to nature-study by all that is lovely in form and color, and fascinating by reason of sound or motion on the still developing earth's surface with which man's life is linked and of which his body is a part.

The popular idea in reference to methods of acquiring geographical knowledge is, no doubt, to traverse unknown lands, make voyages in Arctic and Antarctic seas and scale mountains never before pressed by human foot. Geographical advances, however, are to be made not only by crossing ice fields and climbing mountains, but by excursions into the realm of ideas as well. The science culminates in the study of the relation of life and particularly of man to surrounding physical conditions. While the explorer of new lands gathers facts, the philosophical geographer arranges those facts in orderly sequence, interprets their meaning and deduces from them hypotheses, which have for their purpose the discovery of the laws of Nature. It is the formulating and elucidating of these laws which constitutes the noblest aim of geographical science. This philosophical stage in the growth of geography has but recently been entered upon, and is the one which is to claim the greatest share of attention in the future.

From this point of view it appears that fresh fields for exploration surround the scientific geographer on every hand, and the sea has only just begun to yield up its secrets. Some of the most important advances in geography yet made can be claimed as the fruits of home study rather than resulting from explorations in new lands, although based on and supported by extensive field investigations. The gaining of geographical knowledge at first hand, or geographical research, consists, then, of both journeying and thinking, and the two are inseparable in order to secure the highest results. To the question: What is to be done with the fruits of geographical studies when gathered? the true answer is: Give them away. Sow the seeds of knowledge broadcast in the minds of men, with faith that some of them will germinate there and multiply a thousandfold.

With the change from traversing unknown areas to exploring the domain of ideas, which made geography a science, the sphere of usefulness of the geographical society has been vastly enlarged and new duties placed upon it. Thus far, however, geographical societies do not seem to have awakened to the full realization of the dignity of this new life, and the vast possibilities it opens for their own growth and elevation. It needs no argument to show that it is a duty of a society having the study of the earth's surface for its chosen field, to foster and encourage geographical research in the laboratory and library, in cultivated fields, and amid hills and valleys, just as truly as it is to aid the African explorer or encourage the mountaineer who would scale Mount Everest.

The principal functions of geographical societies may be summarized as being: The encouragement of exploration and research; the holding of meetings for the presentation of information on geographical matters, and eliciting discussion; public lectures; field excursions, etc.; publication of instructive geographical reports, essays, maps, etc.; maintenance of libraries; facilitating personal conferences between men engaged in like explorations or investigations; the stimulating of public interest in matters geographical; and the education of legislators as to the relation of geography to human advancement. Even this suggestive summary does not exhaust the subject in hand; the recognition of work well done, as when a geographical society bestows a medal on an explorer; the assumption of the duties of an executor, as when such a society administers a legacy; the opening of halls for the exhibition of loan collections of various kinds, etc., show that the functions of geographical societies are still wider and more varied. The fact should be emphasized that in the exercise of several, if not all, its functions, the power of a geographical society to do good and enhance the welfare of mankind increases both with the growth of its ideals and its increase in numbers.

There is abundant evidence that by means of co-operation something is gained which is denied the isolated individual, and so far as experience suggests there is no upper limit to the number that can to advantage unite their efforts. At the present time, there are in North America not less than seventeen societies, associations, and clubs, which have geography in some form as the chief bond which unites their members. The distribution of the societies includes, in an east and west direction, Boston and San Francisco, and its range in latitude is from Washington to Quebec on the east, and from San Francisco to Seattle on the west. Of these there are perhaps ten which, as declared by their constitutions, and made evident by their work, can reasonably claim recognition as geographical societies; the remainder are of the nature of social clubs, with geographical features, rather than societies having for their leading aim an earnest desire to increase and diffuse geographical knowledge. The combined active membership of what may be termed *bona fide* geographical societies is over nine thousand, a number which in itself is significant of a wide popular interest in geographical matters particularly among the people of the United States. Each of our geographical societies has its home in a large city, but it is probable, however, that there are many thousands of people outside the cities in which the societies referred to are located who would join similar organizations if it were practicable for them to attend their meetings.

Exploration has been aided by some of our great societies, but research aside from expedition work has not been directly fostered by them. The greatest efforts our societies have made have been in the direction of disseminating geographical information and attracting popular attention to the results explorers and travelers have brought home. During the year 1903, our geographical societies, clubs, etc., held a total of over 60 home meetings, in part scientific and in part popular; conducted not less than 44 public lectures, and engaged in about 16 field meetings. In addition to these direct methods of spreading information, mostly by addresses and lectures, our societies publish on an average approximating 2,000 octavo pages of printed matter each year. These statistics certainly make a favorable showing, and furnish hopeful signs by which to judge of the possibilities of the future, but the quality of the work our geographical societies are doing is difficult of even approximate determination, since there is no generally accepted standard of measurement available.

The lectures delivered under the auspices of our geographical societies must in general be considered good and their influence wide-reaching. The scientific sessions, too, of the societies must be accredited with having added important truths to the world's store of knowledge and with having exerted a beneficent influence on thought and methods of thinking. But the smallness of the assemblies, when the questions bearing on scientific geography are discussed, is discouraging. The publications of our geographical societies, when judged as attempts to popularize geographical knowledge, may in general be considered with truth to lack literary merit. They are merely descriptive and do not usually lead the reader on to think for himself, although it must be stated that they do contain a few papers that are direct and first-hand additions to science. As specimens of the book maker's art, furthermore, their appearance is in general not attractive, and the illustrations are poorly reproduced. The bulletins are not widely known and, although they are exchanged with scientific societies in this and other lands, they do not find their way into public, collegiate or private libraries to the extent that could be wished. The scattering of the efforts of the societies is too great for strength.

As a summary of the defects of our present system, it may be stated that our geographical societies are not only lacking in unity of purpose, but are antagonistic rather than co-operative. Their influence in each case is local, and their aims narrow and ill defined. In no case has research, the true foundation of geography as a science, been made a prominent feature, and never, apparently, has it received direct financial aid or popular recognition. Owing to the local character of the societies in question and the narrowness of their respective habitats, the facilities they furnish for men to become acquainted with their fellow workers are much less than could be desired. But the most glaring failures are evident in the general weakness of the publications issued, and the inefficiency of the means employed for their distribution.

The best remedy for the trouble seems to lie along one of two lines. One plan, which contemplates the reorganization of our geographical societies, provided it can be satisfactorily adjusted to the interests of all concerned, has for its chief feature the union of all the geographical societies of North America with the oldest in the list, namely, the American Geographical Society. Under this plan each society effecting such a union would become a chapter of the home society but retain its own organization and its own property, but unite with the parent society in holding annual meetings and in publishing a monthly magazine.

The other proposes that the several geographical societies now in existence, and such other similar societies as may be organized in North America, while retaining their individual names and autonomy, unite in a brotherhood of societies to be designated by some appropriate name, as for example, the "League of American Geographical Societies." Suggestions more in detail which point the way for securing such co-operation were then presented, it being understood that the first step would be the holding of a convention at which representatives of each society which might desire to join the League should be present and assist in framing a constitution and by-laws.

The management of the League would be intrusted to a council consisting of the president, vice-presidents, secretary, and a body of councilors elected by the constituent societies. The presidents of the local societies would be the vice-presidents of the National League. The functions of the League would be the holding of an annual congress at some center of geographical interest, open to all the members of the affiliated societies, for the purpose of reading and discussing papers, etc., and the publishing of a monthly magazine or other journal to take the place of the publications previously issued by the several affiliated societies. The expense of each annual congress to be borne by the members in attendance, and the cost of the magazine to be shared by the affiliated societies in proportion to their active membership.

The advantage of an annual congress as may be predicted would be, large audiences with wide geographical representation, favorable opportunities for personal conferences and the cementation of friendships, and the encouragement that large and representative gatherings would extend to explorers and investigators to present the best fruits of their labors. To these gains should be added the stimulus such a congress would have in the home cities of the affiliated societies, at which sessions would be held, thus tending each year in an important way to extend the influence and enlarge the membership of some one local society. The greater influence on legislation to be expected from the combined voices of many societies over the efforts of any single local society suggests a practically new field of usefulness to the geographers of America.

The gains to be expected from a concentration of publications are, to a marked degree, expressed by the fact that the proposed magazine, in case all of our geographical societies united in its support, would start with a circulation in excess of ten thousand, not including libraries or subscribers not members of the affiliated societies. With such a vigorous start, rapid growth and a constantly widening influence for many years to come may reasonably be predicted. Perhaps the greatest gain to be hoped for is in the direction of a higher tone and better preparation, that a widely recognized, well edited, well printed and well illustrated magazine would have over for the most part obscure and indifferently printed proceedings, journals, magazines, bulletins, etc., now issued. Again, it may reasonably be expected that an attractive geographical magazine would replace to a considerable extent the popular literary magazines of to-day, and secure a large number of readers outside of the societies from which it derived its main support. A magazine having for its aim the diffusion of all branches of geographical knowledge, would be welcomed by tens of thousands of our school teachers and other intelligent people in isolated communities who are debarred from oral instruction by leaders in geographical exploration and research.

In addition to the richer harvest to be expected from an annual congress of American geographers and a jointly published magazine as just considered, earnest and active co-operation among our geographical societies, as may reasonably be expected from such concentration of energy, should lead to their taking the initiative in several other directions. Among such hopes of the future is the securing of a map of North America on a scale of 1-1,000,000, as a contribution to the map of the world in the completion of which certain European societies are interested. Another desirable undertaking would be the publication of detailed instructions for the use of travelers and others, as to how and what to observe, in reference especially to the securing of the best possible illustrations of the results of known physiographic processes, and the recording of facts which are likely to lead to the discovery of new laws. Again, time and money might well be expended in preparing and publishing a dictionary of geographical terms; a bibliography of geographical literature; in assembling a library of photographs particularly of regions where geographical changes are most active, and in yet other directions.

In proposing the application of modern business methods in the concentration of geographical factories, as our societies may be termed, attention may be directed to the fact that geography more than any other science is best adapted for the purpose of general or popular education. Added to the fascinations of exploration, we now have the equally absorbing results of scientific physical geography, pertaining to the fields through which we walk, the brook whose murmurs have appealed to us since childhood, the waves that beat on the shore where we perhaps spend our vacations, and many other equally familiar scenes. The ability to read the history of the earth at first hand should be within the reach of every civilized man, woman, and child. It is in order to secure to all the people in North America this means of public educa-

* Address as retiring vice-president of the American Association for the Advancement of Science, before the section of Geology and Geography at the Philadelphia meeting, December 28, 1904.

tion, coupled with never ending pleasure and a constantly expanding mental horizon, that our geographical societies are asked to unite their efforts.

[Continued from SUPPLEMENT No. 1514, page 24265.]

ON THE MODERN REFLECTING TELESCOPE, AND THE MAKING AND TESTING OF OPTICAL MIRRORS.*

By G. W. RITCHEY.

XIII. TESTING AND FIGURING PARABOLOIDAL MIRRORS.

THE work of changing a spherical mirror to a paraboloidal one is accomplished entirely by the use of polishing tools, by shortening the radii of curvature of the inner zones, instead of by increasing or lengthening those of the outer zones. The methods of effecting this change of curvature will be described after the methods of testing a paraboloid have been discussed.

Such testing can be done at the center of curvature, by determining there the foci or the radii of curvature of successive zones of the mirror; it may be done at the *focus* of the paraboloid, by the aid of a finished plane mirror which should be at least as large as the paraboloidal one; and it may be done directly on a star. The first two methods named have the very great advantage that they may be conducted without interruption, under the practically perfect atmospheric and temperature conditions of the optical laboratory.

Testing a Paraboloid at the Center of Curvature.—A knowledge of the properties of the parabola enables the optician to compute the positions of the centers of curvature of successive, definite, narrow zones of the mirror, and the surface must be so figured that the radius of curvature of each zone agrees with the computed value. In testing, each zone in succession is exposed by means of a suitable diaphragm, all of the rest of the surface being covered. In practice, two entirely different formulæ may be used, depending upon the position of the illuminated pinhole.

Let F be the focal length of a finished paraboloidal mirror, and R the semi-diameter of any extremely nar-

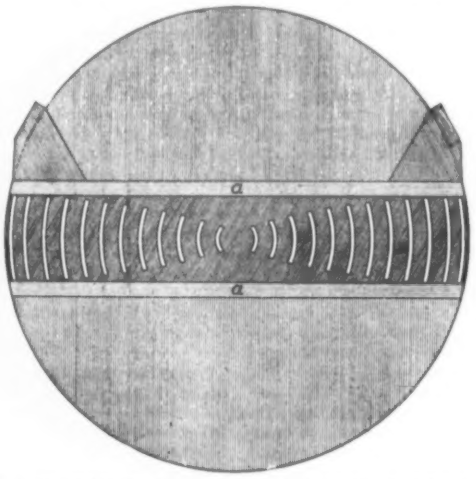


FIG. 7.—DIAGRAM USED IN TESTING A PARABOLOIDAL MIRROR AT ITS CENTER OF CURVATURE.

row zone or ring of its surface, concentric with the vertex or center of the mirror; the normals to this zone cross the axis at a point whose distance from the vertex is $2F + \frac{R^2}{4F}$; hence, if the illuminated pinhole be placed very close to the axis, and at a distance of $2F + \frac{R^2}{4F}$ from the vertex, the rays of light reflected from the narrow zone will form a focus or image in the same plane (at right angles to the axis) in which the pinhole itself lies. This is the simplest formula which can be used, but it is not the most useful in practice.

In testing paraboloids at the center of curvature the writer has always used the following method and formula: The illuminated pinhole remains fixed at the center of curvature of the central parts of the mirror, i. e., at a distance $2F$ from the vertex, where F is the focal length. The intervals, measured along the axis, between the reflected foci of the various zones, are now twice as great as those given by the method described in the preceding paragraph; consequently these foci can now be determined with twice the accuracy which can be attained by that method. Only the rays reflected from the parts of the paraboloid very near to the vertex are now brought to a focus in the plane of the pinhole. If the paraboloidal figure is perfect, the rays reflected from any very narrow zone whose semi-diameter is R are now brought to a focus at a distance $\frac{R^2}{2F} + \frac{R^4}{16F^3}$ back of the plane of pinhole, i. e., at a distance of $2F + \frac{R^2}{2F} + \frac{R^4}{16F^3}$ from the vertex of the paraboloid. The quantity $\frac{R^4}{16F^3}$ is so small in the case of mirrors of moderate size and of ordinary ratios of aperture to focal length that it can be neglected; even in testing the outermost zones of the 5-foot mirror of 25 feet focal length, this quantity is less than 0.002 inch, while the quantity $\frac{R^2}{2F}$ amounts to $1\frac{1}{2}$ inches.

Now let us consider what is the best method of determining the planes of the reflected foci. Draper, Common, and other workers used an eyepiece for this purpose; this serves well for mirrors of moderate angular aperture, but for mirrors in which the ratio of aperture to focal length is as great as 1 to 5 or 1 to 6 this method presents serious difficulties; if narrow zones are used the image in the eyepiece is blurred and indistinct on account of the diffraction effect produced by the edges of the zonal openings in the diaphragm, while if wide zones are used the difference of

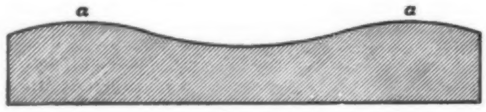


FIG. 8.

focus of the inner and outer parts of a zone is so great that the image shows evidence of marked aberration; with neither narrow nor wide zones can the position of the focus be determined with very great accuracy.

In "Publications of the A. S. P." vol. xiv., No. 87, Hussey gives a formula for the position of the "circle of least confusion" when a zone of given width is used; if Hussey's formula were employed and the pinhole were made very small and round, with smooth edges, it is probable that much greater accuracy could be attained than by the use of an eyepiece in the ordinary way.

The method of locating the reflected foci which is used by the writer is as follows: it is capable of surprising accuracy when the optician has become experienced in its use. The reflected focus of a zone is found with the knife-edge, precisely as the focus of a spherical mirror is found. The knife-edge is moved across the reflected cone from the left; if the left side of the zone is seen to darken first, the knife-edge is inside of the focus; if the right side darkens first, the knife-edge is outside of the focus; when the right and left sides of the zone darken simultaneously, the knife-edge is at the focus of the zone. One advantage of this method is that it is independent of changes of focus of the eye itself; but the great advantage is that very narrow zones or arcs can be used. Diaphragms with zonal openings ¼ of an inch wide serve admirably for mirrors of 10 or 15 feet focal length; indeed the width of the zones which are actually used is considerably less than this; for, on account of diffraction, the edges of the openings in the diaphragms always appear as brilliant lines, even while the illumination near the center of the openings is being cut off by the knife-edge; it is therefore only the illumination near the center that is used in making the comparison.

The diaphragms which I use in this method of testing do not expose entire zones, but only pairs of arcs on the right and left sides of the mirror. Fig. 7 shows the diaphragm which was used in testing in this way the mirror of the two-foot reflector of the Yerkes Observatory. The arcs are cut in a long and narrow strip of thin metal; this is attached to the inner edges of two wooden strips, a; these edges are curved so that all parts of the thin metal diaphragm are nearly in contact with the curved surface of the mirror. The edges of the openings are beveled so as to be extremely thin, and are finished dead-black. Twelve pairs of arcs were used, with mean radii of 1, 2, 3, . . . 10, 11, and 11¾ inches. The openings of these arcs are ¼ inch in width. The foci of the successive zones (except those near the center) can be readily determined by this means to within 1-500 inch along the axis, for a mirror of two feet aperture and of ten or fifteen feet focal length.

Care must be taken when testing in this way that the entire mirror surface is uniformly illuminated by

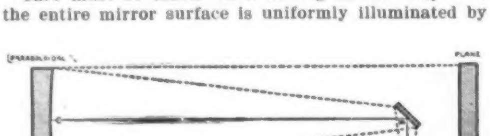


FIG. 9.—TESTING A PARABOLOIDAL MIRROR AT ITS FOCUS.

the cone of light proceeding from the illuminated pinhole; this condition, once secured, is easily maintained, since the illuminated pinhole remains immovable.

I have described at considerable length the methods of testing paraboloids at the center of curvature, because of the importance of the subject, and because this will probably continue to be a favorite method, especially among amateurs. But when testing is done at the center of curvature, even with the extremely accurate method just described, the making of a large paraboloidal mirror of great angular aperture and really fine figure is an exceedingly difficult task. This is due in part to the necessity of very frequent tests, in each of which the foci of a large number of zones must be determined; it is due far more to the uncertainty in determining the exact nature of errors of surface (considering the surface as a whole) corresponding to focal readings which do not agree with the computed values. In the case of mirrors of small or moderate angular aperture, much important information can be gained by viewing the surface as a whole, from the (mean) center of curvature, by means of the knife-edge test; a finished paraboloid, when thus seen, appears to stand out in relief, in strong light and shade, as a surface of revolution whose section is that shown in Fig. 8; knife-edge and pinhole are both at the center of curvature of the zone a; the apparent curve of the surface should be a smooth one. But in the case of a mirror of large angular aperture the change of curvature is so rapid that only a narrow zone can be seen well at one time, i. e., with a given focal setting of the knife-edge.

Testing a Paraboloid at its Focus.—This method was briefly described by the writer in the Astrophysical Journal, November, 1901. It is incomparably more simple, direct, and rigorous than the test at the center of curvature. A well-figured plane mirror, which should not be smaller than the paraboloidal one, is necessary in order that the testing may be done in the optical laboratory. In practice a small diagonal plane mirror is also used, to avoid the necessity of a central hole through the large plane mirror. Both of the plane mirrors are silvered. The arrangement of mirrors is shown in Fig. 9. The diagonal prism is placed at f, with the illuminated pinhole very near the axis; pinhole and knife-edge are in the same plane, at a distance from the vertex equal to $cm + mf$, which is equal to the focal length of the mirror. The paraboloid is now tested as a whole, without the use of zones, precisely as a spherical mirror is tested at its center of curvature.

If F be the desired focal length of the paraboloidal mirror whose semi-diameter is R, then the spherical surface which is fine-ground and fully polished prepar-

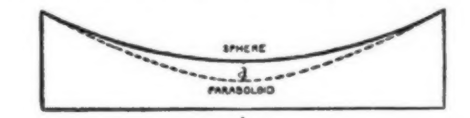


FIG. 10.

atory to parabolizing should have a radius of curvature $2F + \frac{R^2}{4F}$. This is because parabolizing is done by shortening the radii of curvature of all the inner zones of a mirror, leaving the outermost zone unchanged, as shown in Fig. 10; this is a far easier and better method in practice than to leave the central parts of the mirror unchanged, and to lengthen the radii of curvature of all of the outer zones, as shown in Fig. 11.

Let us now suppose that the concave mirror shown

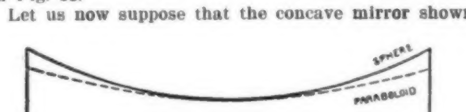


FIG. 11.

in Fig. 9 is a spherical one with radius of curvature $2F + \frac{R^2}{4F}$, where R is the semi-diameter, and F is the distance $cm + mf$, from the center of the mirror surface to the plane of the pinhole and knife-edge. If the spherical surface be now viewed from the point f with the knife-edge test, it will appear to stand out in relief, in strong light and shade, as a surface of revolution whose section is that shown in Fig. 12, the height of the protuberant center depending upon the angular aperture of the mirror. The reason for this appearance is readily seen by reference to Fig. 10. To change the spherical surface to a paraboloid, the protuberant center must be removed by the use of suitable polishing tools, until the surface, as seen with the knife-edge test from the point f, appears perfectly flat, i. e., the illuminated surface darkens with perfect uniformity all over. As the paraboloidal surface nears completion, an elevated or depressed center, a "turned up" or "turned down" edge, or protuberant or depressed zones, can be seen and their character and exact position determined, with precisely the same ease and certainty with which similar irregularities are seen when a spherical mirror is examined at its center of curvation with the knife-edge test.

It should be noticed that even when the pinhole and reflected image are very near each other, as they should be, yet both may be far out of the axis of the paraboloid, if the mirrors are not properly adjusted or collimated; when this is the case the mirror surface, when seen with the knife-edge test, does not appear as a surface of revolution, and cannot be properly tested. The mirrors may be collimated by the following method, thus insuring that the pinhole and reflected image are both extremely near the optical axis.

The mirrors are set up approximately right by measurement. A ring about an inch in diameter, with two fine threads stretched diametrically across it, one vertical, one horizontal, is set up near the plane of the illuminated pinhole, the intersection of the threads marking the desired position of the optical

* Reprinted from vol. xxxiv, Smithsonian Contributions to Knowledge.

axis. A light, stiff ring is made, which fits closely over the edge of the paraboloidal mirror, at the front; this ring can be slipped on and taken off as required. Two very fine bright wires are stretched diametrically across this ring, one vertical, one horizontal; these wires should be as close as possible to the face of the mirror; their intersection marks the position of the center or vortex of the paraboloid. Two fine short lines, one vertical, one horizontal, are scratched with a fine needle-point at the center of the silvered face of the small diagonal plane mirror. The eye is now placed about 3 feet outside of the plane of the crossed threads, and an assistant changes the inclination of the small plane mirror, by means of three adjusting-screws at its back, until the intersections of the threads, of the scratches, and of the wires are all seen in exact coincidence. The assistant next changes the inclination of the paraboloidal mirror (by means of three adjusting-screws at its back) until, with the eye in the same position as before, the intersection of the threads, the intersection of the wires, and the reflection of the intersection of the threads seen in the paraboloidal mirror, all appear in exact coincidence; the position of the axis of the paraboloid has now been defined. No attention is paid to the large plane mirror in this part of the work. The illuminated pinhole is now placed in position, and the large plane mirror is adjusted (by means of three adjusting-screws at its back) until the reflected image falls in the right position with reference to the axis and pinhole.

The frame which carries the paraboloidal mirror can easily be so designed that this mirror can be removed and replaced repeatedly, while figuring it, without sensibly disturbing the adjustments.

The difficulties of making short-focus paraboloidal mirrors of fine figure are so greatly reduced when this method of testing is used that I believe that the general adoption of this method by opticians would lead to such improvements in results as to bring about a marked advance in the usefulness of reflecting telescopes. The making of the large plane mirror which is necessary in this test becomes so simple and certain when the methods of testing and figuring described in the preceding chapter are used, that I have no hesitation in saying that when a large paraboloidal mirror of short focus and of the finest attainable figure is to be made, it is economical to make a plane mirror of the same size, with which to test it, if one is not already available. The concave mirror is first figured spherical and is used thus for testing the plane mirror while the latter is being figured; the plane mirror is then used in testing the concave one during the parabolizing of the latter. Both the plane and paraboloidal mirrors

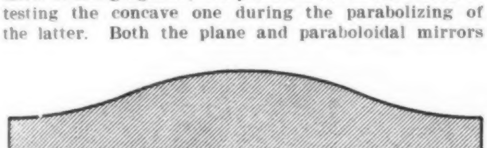


FIG. 12.

are then used in testing the small (convex) hyperboloidal mirror while the latter is being figured.

Testing a Paraboloid on a Star.—With this method the mirror surface, as seen with the knife-edge test, presents the same general appearance as in testing in conjunction with a large plane mirror; in the latter test, however, errors of surface are seen in greater relief, because the effect of such errors is doubled on account of the two reflections from the paraboloid. In addition, it is impossible to overestimate the advantage of being able to test as often as is desired, in the optical laboratory, where atmospheric and temperature conditions can be controlled perfectly, and where the mirror does not have to be removed from the polishing machine in order to test it. In testing on a star it is seldom indeed that atmospheric conditions are sufficiently fine to allow any except the larger errors of surface to be seen.

Changing a Spherical Surface to a Paraboloid.—As before stated, this is accomplished by shortening the radii of curvature of all of the inner zones of the surface, leaving the outermost zone unchanged (see Fig. 10). There are two distinct methods of accomplishing this: (1) by the use of full-size polishing tools, the rosin surfaces of which are cut away in such manner as to give a large excess of polishing surface near the central parts of the tool; (2) by the use of small polishing or figuring tools worked chiefly upon the central parts of the mirror, and less and less upon the zones toward the edge.

(1) *Parabolizing with Full-Size Tools.*—The rosin surface can be trimmed in a variety of ways to give a great excess of action on the central parts of the mirror. Fig. 13 shows one of the best forms of tool for this purpose, the shaded parts representing the rosin surface, coated with wax. The form of the edges of the rosin-covered areas can be altered as desired, and thus the amount of action on any zone can be in some measure controlled. Length of stroke and amount of side-throw are also very important factors in controlling the figure of the mirror. Tools of this kind serve admirably in parabolizing mirrors up to 36 or 40 inches in diameter, when the angular aperture is not very great.

(2) *Parabolizing with One-Third-Size and Smaller Tools.*—In the case of very large mirrors, when full-size tools are almost unmanageably heavy, and in the case of mirrors of great angular aperture, in which the departure from a spherical surface is great and is effected with difficulty with full-size tools, one-third-size and smaller figuring tools may be used. The machine should invariably be employed in this work, the transverse slide being used to place the tool in succession upon the various zones. In order to preserve the surface of revolution the setting of the transverse slide should be changed only at the end of one or more complete revolutions of the glass. The rosin squares of the small tools should be somewhat softer than usual, so that the surfaces of the tools can accommodate themselves slowly to the slightly different curvatures of the successive zones. The squares around the edges of the tools should be trimmed, as before described, in order to soften the action of the edges. The mirror should be tested very often, and the utmost care taken to keep the apparent curve of the surface, as seen with the knife-edge test, a smooth one, i. e., free from small zonal irregularities, at all stages of the parabolizing; this is not extremely difficult when the optician has become experienced in the use of the transverse slide.

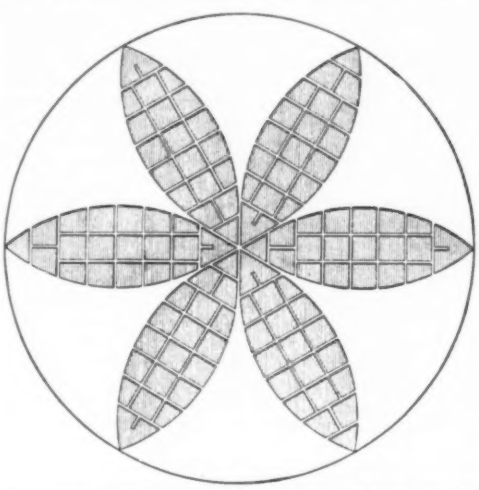


FIG. 13.—FULL-SIZE POLISHING TOOL FOR PARABOLIZING.

The mirror of the 2-foot reflector of the Yerkes Observatory, which has a focal length of only 93 inches, was parabolized in this way by the writer. Two small tools were used, of 6 and 8 inches diameter respectively. The actual difference of depth, at the center or vertex of this mirror, between the paraboloid and the nearest spherical surface is almost exactly 0.0004 inch. This difference is unusually large in this case, on account of the exceptionally great ratio of aperture to focal length. This difference varies, in different mirrors, as the fourth power of the diameter of the mirrors, and inversely as the cube of the focal length. In the case of Lord Rosse's great mirror, in which the aperture is 6 feet and the focal length 54 feet (ratio 1 to 9) the corresponding difference at the center is only 0.0001 inch, very nearly. In the case of the 5-foot mirror of the Yerkes Observatory, of 25 feet focal length, the corresponding difference is about 0.0006 inch. This gives some idea of the actual amount of glass which must be removed by the figuring tools in parabolizing.

XIV. TESTING AND FIGURING CONVEX HYPERBOLOIDAL MIRRORS.

The methods of figuring and rigorously testing convex hyperboloidal mirrors are now so thoroughly developed that the reflecting telescope can be regarded as a universal photographic telescope of the highest class, capable of giving, at the focus of the paraboloidal mirror, of large angular aperture, the finest photographs now attainable of large and excessively faint objects such as the nebulæ in general; while by the addition of a small convex mirror a great equivalent focal length is obtained for the photography of bright celestial objects requiring large scale, such as the moon, the planets, the dense globular star clusters, and the annular and planetary nebulæ. The convex mirror of course serves as an amplifier, and possesses the great advantages over a lens used for this purpose that the perfect achromatism and the high photographic efficiency of the reflector are retained, and that the mechanical arrangements are very compact and economical. In order to give perfect definition the convex mirror must be an hyperboloidal one.

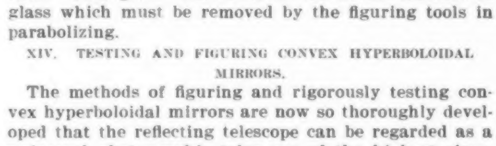


FIG. 14.

The writer has recently made two convex mirrors of different curvature, for use with the 2-foot reflector. These give equivalent focal lengths of 27 and 38 feet respectively.

Fig. 14 shows the arrangement of mirrors employed in the 2-foot reflector when used as a Cassegrain; a small diagonal plane mirror is used at m, to avoid the necessity of a hole through the center of the large concave mirror. P is the paraboloidal mirror, with its focus at f; H is the hyperboloidal mirror, the secondary focus or magnified image produced by the combination being at F: the point c is the center of the hyperboloidal surface. Calling the distance $fc = p$ and the distance $cm + mF = p'$, then $\frac{p'}{p}$ represents the amount of amplification introduced by the convex mirror. The radius of curvature R of the spherical surface to which the convex mirror is ground and polished preparatory to hyperbolizing is found with sufficient accuracy for all practical purposes by the formula

$$\frac{1}{p} - \frac{1}{p'} = \frac{2}{R} \text{ whence } R = \frac{2\,pp'}{p' - p}$$

For example, let the focal length of the paraboloidal mirror P, Fig. 14, be ten feet; let $fc = p = 2$ feet and $cm + mF = p' = 8$ feet. Here $\frac{p'}{p} = 4$; the image of the moon or other celestial object produced at F is therefore four times larger in diameter than it would be at f, the focus of the paraboloid; and $R = \frac{2\,pp'}{p' - p} =$ 64 inches.

The method of testing the convex mirror while hyperbolizing it is shown in Fig. 15. The illuminated pinhole is placed very near the axis at F. The diverging cone of light strikes the small plane mirror, then the convex, then the large paraboloid, whence if all of the mirrors are finished and are well adjusted or collimated, the light is reflected in a parallel beam to the large plane; returning, the rays are brought to a focus very near the axis of figure and in the plane of the illuminated pinhole. All of the mirrors except the convex one are silvered. The convex spherical surface with radius of curvature R, as above described, when viewed with the knife-edge test from the point F, presents the same general appearance of a smoothly curved surface of revolution, in strong light and shade, which a paraboloidal surface presents when similarly viewed from its center of curvature (see Fig. 8). All that is necessary to produce the hyperboloidal surface is to soften down, with suitable polishing tools, the apparent broad protuberant zone between the center and edge, until the mirror, as seen from F, appears perfectly flat; i. e., until the illuminated surface is seen to darken with absolute uniformity all over when the knife-edge is moved across the focus. This hyperbolizing may be done with small local or figuring tools, or with a full-size tool so trimmed as to give an excess of action on the broad zone a, or (what is usually best) by a combination of the use of both kinds of tools.

As in the case of the paraboloid, it is necessary in

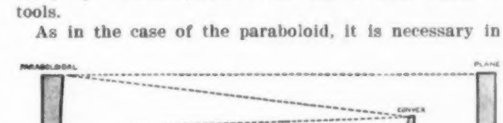


FIG. 15.—DIAGRAM ILLUSTRATING TESTING OF HYPERBOLOIDAL MIRROR.

this test that all of the mirrors be lined up or collimated with care; otherwise the surface of the convex mirror will not appear as a surface of revolution, and cannot be properly tested. The axes of the paraboloid and hyperboloid must coincide, and the face of the large plane mirror must be at right angles to these axes. These adjustments are made by means of an extension of the method of collimation described in the preceding chapter. First the paraboloidal mirror is adjusted so that its axis intersects the hyperboloid at its exact center or vertex; in making this adjustment fine threads are stretched diametrically across the cell of the convex mirror, this mirror being removed during this part of the adjustment. Next, the small diagonal plane is adjusted for inclination, care being taken that the intersection of the lines scratched in its film is placed in the axis of the paraboloid. Then the convex mirror is adjusted for inclination, by reflection. Finally, with the illuminated pinhole in place, the large plane mirror is adjusted, as previously described.

(To be continued.)

WHAT GRADE OF CREOSOTE OIL IS BEST FOR PRESERVATIVE TREATMENT?

THE Bureau of Forestry is making exhaustive tests to determine the best grade of creosote oil for use in wood perservative treatment. The supply of the kinds of timber which are naturally most resistant to decay is diminishing so rapidly that substitutes will have to be found within a few years at furthest. Already the railroads are face to face with a tie famine from the exhaustion of the woods hitherto most used, especially white oak. There are plenty of substitutes, but they all decay so fast in their natural state that though their first cost is low their use is very expensive. In Europe this difficulty was met long ago by devising methods of artificial preservation by which for example, a beech tie, which if untreated will decay in from four to five years, is made to last thirty years or more. This result is obtained by impregnating it with creosote oil.

Other preservative materials are in experimental use in this country, but none gives more promising results in the increased length of service secured. At present most of the creosote oil for this purpose is being obtained from European sources. Quantities sufficient for our use are produced in this country,

but the grades are so many and, for the most part, so inferior that they are but little used.

About 1,000 samples of this oil, both native and European, have been tested by the bureau to determine what grade gives the best results. This involves finding out the grade of oil which both most readily enters the wood and also stays in the longest time. The first will hasten and cheapen the process, the second will insure the greater permanence of the treatment.

The necessity for wood preservative treatment is beyond the theoretical stage. It is a question of recognized and vital importance, especially to the railroads and telegraph and telephone companies, whose bills for constant renewals of ties and poles are enormous. The oil tests the bureau is making are therefore of large and immediate practical value.

INFLUENCE OF BORIC ACID AND BORAX ON DIGESTION AND HEALTH.

A bulletin of the Bureau of Chemistry, now in press, is the first of a series of monographs from that bureau embodying investigations made in accordance with the following authority contained in the act of Congress making appropriations for the Department of Agriculture, to wit: "To enable the Secretary of Agriculture to investigate the character of food preservatives, coloring matters, and other substances added to foods, to determine their relation to digestion and health, and to establish the principles which should guide their use."

These investigations were commenced in the autumn of 1902. Previous to their beginning a careful study of similar work done in this and other countries was undertaken, and some of the laboratories where this work had been carried on, notably the laboratory of the Imperial Board of Health of Germany, at Charlottenburg, were visited and the method of experiments investigated. The plan finally decided upon was to secure the voluntary services of a number of young men who would undertake to try the effect of the added substances upon their digestion and health, to make the necessary observations, and to submit themselves to the rigid analytical control which such a series of investigations required.

The number finally selected for experiment was twelve, as this was found to be about the maximum number which could be cared for with the analytical and culinary facilities afforded by the Bureau of Chemistry. A kitchen and a dining room were fitted up in the basement of the bureau and in December, 1902, the actual experimental work began and it continued, in the case of boric acid and borax, until July 1, 1903. The work was so divided that no one of the young men under observation was required to submit himself to the rigid control necessary to the conduct of the work more than one-half of the time. The men selected were taken partly from the force of the Bureau of Chemistry and the rest from other divisions and bureaus of the Department of Agriculture. Each one was required to subscribe to a pledge to obey all the rules and regulations prescribed, and to abstain from all food and drink during the period of observation save that which was given him in the course of the experiment. Careful medical inspection of each of the members of the experimental class was secured both directly and by collaboration with the public health and marine hospital service. The details of the work, both analytical and medical, are found in full in the bulletin above mentioned which is now in press.

By reason of the provision of an existing law which forbids the publication of more than 1,000 copies of any bulletin containing more than 100 pages, Bulletin No. 84 can not be supplied for general distribution. In order that the data of a popular nature therein contained may receive a wider publicity, a circular, No. 15, which will soon be ready for distribution, has been prepared, presenting in a condensed form the principal details and the general conclusions of the bulletin, omitting the tabular statements and strictly technical part of the text. This circular should be asked for instead of the bulletin.

A summary of the results of the investigations, omitting all technical and analytical detail, is as follows:

(1) Both boric acid and borax, when mixed with the food, are excreted from the body chiefly through the kidneys, about 80 per cent of the total amount exhibited being recovered in the urine. The rest of these bodies is excreted chiefly through the skin with the perspiration. Only traces of them are excreted in the feces. These facts show that these bodies are almost if not quite all absorbed into the circulation from the intestinal canal.

(2) When borax or boric acid is administered in the food, it appears in traces in the urine in a very short time, but if equal quantities of this preservative be administered daily, the maximum quantity excreted in the urine does not appear until about the third day. After that if the same quantities be continued equivalent quantities are excreted from day to day. These facts show that there is not any great tendency to the accumulation of these bodies in the system beyond what would be given over a period of about three days, and even the whole of this is not found in the body at once, as small portions of it, gradually increasing in quantity, begin almost immediately to be excreted after exhibition.

(3) The most convenient method of administering this preservative is by inclosing it in capsules. When mixed directly with the food it tends to give the person eating it a dislike for the food in which the borax is found, due largely to the mental attitude rather than to a bad taste or flavor.

(4) When boric acid or borax equivalent thereto, in small quantities not exceeding a half gramme per day, is given in the food no notable effects are immediately produced. If, however, these small doses be continued for a long while, as for instance in one case 50 days, there are occasional periods of loss of appetite, bad feeling, fullness in the head, and distress in the stomach. These symptoms, however, are not developed in every person within the time covered by the experiment, for some are far more sensitive to the action of these bodies in small quantities than others. There is no tendency in such cases to the establishment of diarrhea or of diuresis, though there is a slight tendency to increase to a very small extent the amount of water in the feces. There is, however, no measurable tendency to increase the volume of the urine.

(5) When boric acid, or borax in equivalent quantities, is given in larger and increasing doses there is a tendency to the somewhat rapid development in a more accentuated form of the symptoms above described. The most common symptom developed is a persistent headache, a sense of fullness in the head, with a clouding to a slight extent of the mental processes. When the doses are increased to 3 grammes a day these symptoms are established in a majority of the cases, but not in every case. They are also sometimes attended by a very distinct feeling of nausea and occasionally by vomiting, though the latter act is rarely established. There is a general feeling of discomfort, however, in almost every case, but the quantities required to establish these symptoms vary greatly with different individuals. In some cases very large quantities may be taken without the establishment of marked symptoms, while in other cases from 1 to 2 grammes per day serve to produce in a short time feelings of discomfort and distress.

(6) The specific action of the boric acid and the borax upon the digestive processes is not very well marked. There is but little apparent disturbance in the process of digestion or assimilation. But there is a slight tendency to decrease the proportions of the food which are digested and assimilated, and thus to cause the excretion of larger quantities of undigested materials in the feces. This action, though it may be traced definitely when large numbers are submitted to experiment, is not of a character to cause any very serious consequences. It is, moreover, not marked enough to warrant the statement that the administration of these bodies in small quantities causes a distinctly unfavorable effect upon the processes of digestion and assimilation, except when its use is long continued.

(7) The effect of the administration of borax upon the weight of the body is very well marked. As its continued exhibition decreases the desire for food, interferes somewhat with the digestion of the food in the alimentary canal, and produces, in certain cases, persistent headache, bad feeling, and discomfort in the region of the stomach, its final effect in diminishing the weight of the body is not doubtful. The compilation of the weights of the body obtained during the whole period of the observations shows a slight tendency to diminish the weight of the body during the administration of the preservative. This tendency becomes so well fixed that it is not entirely eliminated for several days after the administration of the preservative ceases. In the after periods extending in some cases for ten days, and during which time the subject was kept under observation after the administration of the preservative ceased, there was not a uniform nor even a general recovery of the original weight and of the original condition. Any effects produced by the administration of the borax do not extend to any considerable period of time, and apparently no permanent injury to any one of those experimented upon is produced.

(8) No conclusions were reached in regard to smaller quantities than half a gramme per day of the preservative, and, therefore, any statements in regard to the administration of smaller quantities must be based largely upon the results obtained with the quantities actually employed. It is reasonable to infer that bodies of this kind not natural to nor necessary in foods which exert a marked injurious effect, when used in large quantities for short periods of time, would have a tendency to produce an injurious effect when used in small quantities for a long time. The general course of reasoning, therefore, would seem to indicate that it is not advisable to use borax in those articles of food intended for common and continuous use. When placed in food products which are used occasionally and in small quantities it seems only right, in view of the above summary of facts, to require that the quantity and character of the preservative, that is, whether borax or boric acid, be plainly marked so that the consumer may understand the nature of the food he is eating.

(9) The use of borax or boric acid as an external application to cured meats to preserve them in a proper condition during shipment to foreign countries when the use of such preservatives is not prohibited in such countries and when it is especially asked by the purchasers that they may be used, is a question which is not to be decided upon the data which have been obtained. Inasmuch as it is evident that in cured meats the processes of absorption and diffusion will be very much restricted, it is evident that unless the shipment of the product in question extends over a long period of time there could be no very great penetration of the preservatives to the interior of the package. The quantity of borax thus introduced into the food product would be minimal and the desirability or undesirability of its presence would be a question which should be left solely to the decision of the authorities in the countries to which the product is sent.

(10) The convincing justification of the use of boric acid and borax for domestic food products must lie in the possibility of proof on the part of those using them that the food products in question if not preserved in this manner would develop qualities far more injurious to health than the preservatives themselves.

(11) While many of the individual data obtained are contradictory, the general results of the investigation secured by combining into single expressions all the data relating to each particular problem studied show in a convincing way that even in doses not exceeding half a gramme (7½ grains) a day boric acid and borax equivalent thereto are prejudicial when consumed for a long time. It is undoubtedly true that no patent effects may be produced in persons of good health by the occasional use of preservatives of this kind in small quantities, but the young, the debilitated, and the sick must not be forgotten and the safe rule to follow is to exclude these preservatives from foods for general consumption.

ARE THE STARS INHABITED?

By Alexander W. Roberts, D.Sc.

This, after all, is the supreme question of astronomy. Compared with it all other inquiries dealing with the size, distances, movements, composition, and evolution of the heavenly bodies seem somewhat trivial and uninteresting. In one sense, what does it matter to humanity how big the stars are, in what depths of space they swing or circle, whence they have come, and whither they are going?

Could we answer all these questions with a precision absolutely faultless, we should after all be in possession of only a few numerical facts. We determine the distance of the sun, say to a mile, and there is an end of the matter. We exhibit his path through space with unerring accuracy, and there that inquiry also terminates. We place a measuring-line round the sun's circumference, cast his great bulk into scales, analyze his light-pulsations in a physical laboratory, and the problems involved are of the same human interest as measuring, weighing, and testing a piece of coal.

But let it be known to-morrow that men have been discovered on Mars, that, moreover, we have been able to interchange ideas with them, and all the world would be agog at the news.

Who are they? What are they? Are they creatures like ourselves—loving, hating, blessing, cursing, hoping, fearing; some vicious, others virtuous; a few wise, many foolish; a minority rich, a majority poor? Does sin and sorrow, sickness and death, throw a shadow over Martian homes as over those of earth?

What a tumult of tossing thoughts would agitate our little world! What a new realm of ideas would be opened up for man's conquest! At street corners and in the clubs, in shops and in churches, on trains and on steamers, in great cities and in lonely villages, nothing would be spoken about but the stupendous discovery. Theologians, scientists, philosophers, carters, cobblers, and kitchen maids, all would have something to say on a matter of such world-wide moment.

Of world-wide moment! for the fact would touch humanity intimately. It might mean the destruction of much of the world's storehouse of garnered experience. To know what another race, a race so differently conditioned from ours, did and said and thought would of necessity revolutionize many of our most cherished conceptions. Who knows what scientific, political, and social views would have to go by the board the day our world received an intelligible message from Mars? There is indeed all the possibility of catastrophe in the discovery of other worlds than our own.

For my own part, I can conceive of no discovery that the future may hold hid in its lap so calculated to alarm and agitate the world as this, that human beings, men of like passions with ourselves, exist outside the domain of earth's sovereignty. Even the remote possibility of such being true startles the human mind.

Seeing, then, that the question is one of such grave interest, to seek to obtain some kind of answer to it is well within the range of legitimate human desire.

Now, we could straightway give one of two simple yet mutually antagonistic answers to the question which stands as the title of this paper, "Are the stars inhabited?"

We might dismiss the subject without further inquiry, averring that we have no proof whatever, not even the shadow of a shadow of proof, that life, either in its highly organized forms or in its most rudimentary state, exists on a single planet, satellite, or star that shines in our midnight sky. Accordingly it is futile wasting time and words in argument for or against men on Mars or on any other planet. After we have written much and thought more on the matter, are we any nearer the solution of the question? We are not, for the very conditions of the inquiry place it, happily for the peace of mankind, beyond the reach of an immediate solution. Our appliances are too limited in range and power, our minds too finite, to deal with a problem so infinitely complex.

We might as well go puzzling our brains as to what the other side of the moon is like. Certain of her secrets Nature has hidden from the ken of men in her

innermost chambers, and it is useless to try to wrest them from her. Thus some reason.

On the other hand, it is urged that in a boundless universe there are boundless possibilities; that there is not a single star shining in the sky but *may* have a retinue of life-laden planets circling round it. To regard the whole universe as a vast garden ground teeming with rich and abundant life, manifold in form, persistent in vitality, is held to be not only a noble but a true conception of nature.

To those to whom this view appeals, it seems unreasonable to imagine the majestic suns of space wasting all their light and heat and force on the barren emptiness around them. It seems much more in accordance with the fitness of things to suppose that the warmth and brightness and power which streams out in such prodigal and bountiful richness from millions of glowing suns are laid under tribute by circling worlds, on whose surface, as on our own, men live and love and die.

Such a conception fills boundless space with illimitable worlds, each world thronged with a great humanity. What of that? Why should we give to ether and matter, hydrogen and iron, common clay and common salt, the run of the wide universe from shore to shore, and shut up the noblest thing we know—man—to a parcel of ground no bigger than a dust-speck! Thus others reason.

Now, these two views of the question under consideration are mutually antagonistic. They arise from two extreme ways of regarding scientific truth; excluding all conceptions that we cannot uphold by the most direct and indisputable evidence, or including as possible all that our mental and moral sense does not distinctly condemn as untenable.

There is a middle and a much more hopeful way of dealing with this or any other difficult question. Let us use both our reason and our imagination; let us draw upon facts and fancies. The one will correct and stimulate the other.

Are the stars inhabited? There are four considerations which bring us at once into close grips with the question. These four considerations are: (1) What about gravitation? (2) What about density? (3) What about temperature? (4) What about atmosphere? Take the first consideration. Men cannot exist on any planet or star if their feet are chained to the ground by the compelling force of gravity. How absurd to imagine a man carried about from place to place by a steam crane! But this is just what would happen in Sirius or Arcturus or Aldebaran if by any chance an earth-born creature reached its shores, and, having reached them, desired to set forth on a voyage of discovery before he returned to his familiar, homely, kindly earth.

On the sun's surface, for example, an ordinary man would weigh over two tons. His clothing alone would weigh more than a hundredweight. He could with ease play golf in a solar drawing-room (ten yards would be a magnificent drive), and ordinary field-tennis would take the place of ping-pong. There are always, in nature, some compensations for loss.

Now, the sun is by no means a large star. It is, compared with many other stars that could be named, a very small star.

There are stars, indeed, both bright and dark, on whose outer surface a year-old baby would weigh a hundred tons.

Life as we know it, life with every part finely balanced, every heart-throb carefully measured, every nerve and muscle perfectly adjusted to meet earth's stress and strain, would be utterly impossible under such conditions as we have indicated.

The delicate mechanism of the body would go to pieces instantly under such a stupendous pressure. It would be crushed into a shapeless mass by the sheer force of its own weight.

There is no star—we exclude planets—at present known to astronomers where man could live without dire distress or move without enormous exertion. And this holds equally good of both bright stars and dark.

What about any planets which may circle round the stars as central suns? As yet no planet or group of planets has been discovered revolving round a star, for the simple reason that we have no means of making the discovery. It would require a telescope with a tube stretching from Edinburgh to the moon and an object-glass as large as the earth itself to reveal the disk of a Sirian planet, if any such existed.

But since the sun is also a star, what is true of it may be true of every yellow star that shines in the sky. On this understanding let us consider the force of gravity on the surface of those planets that we know about, those that are companions to our earth in its circuit round the sun.

On the surface of this earth everything weighs more than three times what it would do if it were transferred to the surface of Mars. Thus a man on Mars would be three times as swift, three times as strong, and therefore able to do three times as much work as he could on this toil-laden earth. This fact has frequently been brought forward as an explanation of the so-called Martian canals. On Mars digging even a Panama Canal would not tax unduly the energies of a municipality, not to speak of the resources of a State.

On Mercury the same man would be half as heavy, and on Venus, Uranus, and Neptune almost as heavy, as he is upon the earth's surface.

If by any means he found himself traveling in Saturn, a penny in the slot at a Saturnian railway station would reveal to him that he was only a few pounds heavier—nothing to speak of—than he was when last he weighed himself at some terrestrial railway station. On the planet Jupiter, if he were a man of ordinary proportions, he would turn the scale at a quarter of a ton.

The opposite of this is true of the moon. Over the glassy surface of one of the lunar planes a young athlete could bound with a speed equal to that of an express train, for his weight at most would be only twenty or twenty-five pounds.

Given lungs that require no air, blood that will not freeze or boil under the greatest extremes of temperature, lips that are never parched with thirst, bodies that crave for no sustenance, and a lunar holiday would be a perfect delight. It would not be globe-trotting; it would be globe-leaping and vaulting and bounding and flying. But human lungs do need air, warm blood readily chills, and water and food we must have or we perish.

However, the consideration before us at this point is weight or pressure, and we have to consider the question: Would the force of gravity be so powerful on some bodies and so weak on others that a human body would be crushed in the one case and go sprawling helplessly through space in the other? The answer is, that no life that we are acquainted with could stand the enormous pressure which exists on the surface of the stars. As far as gravity alone is concerned, men could walk without inconvenience on any of the planets with the single exception of Jupiter; on this giant planet movement would be impossible.

We come now to the second consideration: What about density?

Man is so constituted both physically and morally that he must have something solid, something unyielding, beneath his feet. He cannot walk on clouds or on vapor, or find a sure foothold on the shores of a misty dreamland.

No planet or star, therefore, the density of whose substance is equal to that of air or even water can be the abode of man as we know him.

Now, the density of the great majority of the stars is less than that of water; some even are composed of matter "light as air."

Therefore, if a man tried to walk on the surface of these gaseous orbs he would certainly sink into the depths beneath him as a stone would that sought a lodgment on the surface of a placid lake, or an Alpine traveler who essayed to step forth on to the rolling sea of cloud that hid from his view the abyss below.

The same holds true of three of the planets, Saturn, Uranus, and Neptune. The density of these three planets is less than that of water.

With regard to these three bodies, ingenuity has suggested a solid shell covering a liquid interior. The idea is an utterly impossible one. The tidal strain on the liquid core of such a planet would rend the confining shell into fragments. Further, we cannot conceive of a star or a planet growing concentrically more dense from the center outward, like a bamboo stalk. Dynamical laws demand an opposite development.

On Saturn, Uranus, and Neptune there are no towered cities, no busy hum of men, no meadowland sweet with flowers, no pleasant lanes or crowded streets; nothing but rolling waves of liquid matter, ever surging and swelling under a dismal and practically sunless sky. From pole to pole there stretches a gray, heaving plain, with neither rock nor shore to break the vast, weary monotony.

On Jupiter the conditions of life are not much better. His outer crust must be of the consistency of mud or treacle. From this semi-molten sphere there rise also great volumes of vapor which the swift rotation of the planet drives into equatorial cloud-belts.

Should an inhabitant of earth land on Jupiter, his condition would indeed be a deplorable one. A man trying to skim over a lake of boiling pitch would be in happier circumstances. Now, there may be beings to whom the heated fumes of half-molten iron, sodium, magnesium, and carbon are as zephyrs that have drawn their sweetness from southern spice-gardens; but he must be indeed a strange mortal who can enjoy all this tethered to a Jovian rock lest a cyclone sweep him out into the night. Can life be possible on such a planet?

Mars? There is little fear that anything less bulky than an elephant should sink through the Martian soil; for on Mars the rocks are one-half the density of those that form the ribs of the earth. As far as density is concerned men could quite easily live and work on Mars.

The same holds good of Mercury, whose density is not much different from that of the earth.

It has always appeared to me remarkable that the minds of men have not been drawn more to Venus than Mars as a possible world.

Not only is it almost the same size as the earth, but it is practically of the same density—that is, the materials which go to form our earth are probably those of which Venus is also composed.

An inhabitant of the earth would, therefore, not find the morning or the evening star a strange dwelling place.

We come now to the third consideration: What about temperature?

I think we may leave the stars out of the question in this consideration. Men are not salamanders; and a temperature ranging between one hundred thousand degrees and one million degrees is sufficient to prevent, even in our dreams, a voyage to Sirius or Canopus or any other giant star.

What, then of the planets, of the moon? Saturn, Uranus, and Neptune, especially the latter two, are so far distant from the sun that the chill of space lies round them like a shroud.

It is possible that some primeval heat remains conserved in their vast bulk, so that there streams up from the Uranian and Neptunian soil enough warmth to compensate for that lost by reason of their distance from the sun. But, alas! even in such circumstances, what worlds to live in the two outer planets must be, with no sun to brighten the day and no moon to beautify the night; always gloom, always darkness, always sadness; no changing seasons, no pleasing variations of climate, no dawn dappling the sky, no long-drawn-out twilight hours; a gray world, a cold world, a sad world, an uninhabitable world! On Mercury the opposite of nearly all this is true. Fierce, scorching sunlight turns the Mercurian sky into a blinding dome of light; the sun is no longer beneficent and lifegiving, but baleful and terrible in its destructive power. There is no place for man in a world like this.

Only on three of the planets would it be possible to live if we consider the sun's heat alone—namely, Jupiter, Mars, and Venus. With regard to temperature, Mars and Venus cannot be very different from our own earth. Indeed, on Venus there would be no inhospitable polar regions forever barred against man's dominion, while the equatorial realms of the planet would not be more than ten degrees warmer than the average tropical temperature of terrestrial lands.

Thus, as far as extremes of heat and cold go, man could enjoy life to the full on either Mars or Venus.

We have only now the fourth consideration left: What about air?

Man must have air. On the moon he would instantly die, for not a trace of atmosphere exists on that dead world. Would he have sufficient air on the other planets? We cannot tell.

There is an atmosphere of some kind on all the planets, but we are unable to say of what gases each atmosphere is composed. Probably those on Venus and Mars are similar to the terrestrial atmosphere both in composition and density.

A grave uncertainty, however, surrounds the whole question, an uncertainty which will not be removed until we are able to determine from reflected light the character of the reflecting surface. This at present is beyond our knowledge.

Now, to sum up the whole matter, we find that the conclusions of modern astronomy on the possibility of the stars being inhabited are somewhat as follows:

It is impossible for any form of life to exist on a lucid star. It is possible for life of a kind to exist on some of the dark stars that are known to astronomers. This possibility, however, is extremely remote.

Lucid stars may be accompanied by planets forming stellar systems similar to our solar system, and life might exist on such stellar planets. There is nothing in the wide range of astronomical fact to negative such a possibility; but there is also nothing in the whole circle of astronomical knowledge to affirm the existence of such planetary bodies.

It is utterly impossible for life to exist on the moon. Novelists may discover holes in which selenites hide, but science turns these holes into graves and buries the dead selenites in them.

It is beyond our conception how life can exist on any of the planets (other than our own) except two. The justification of this conclusion are the facts stated in this paper. The two planets on which life might exist are Mars and Venus. The soil of these two worlds is solid enough to form a footing for man and his inventions. The air of both, though rare, is probably sufficient to enable him to live and work. There are times and seasons, day and night, on both planets; certainly on Mars. There is also mist and rain, sleet and snow, on both planets. Again we say, certainly on Mars.

There are high mountains and deep valleys on Venus, vast plains and gigantic waterways on Mars.

There is much that would go to make a habitable world on both planets, only as yet no trace of life in any of its forms has been discovered on the surface of either Mars or Venus. It must be remembered, however, that the outlines of no object less than fifty miles in diameter can be clearly discerned on the nearest of the planets, and thus human beings may exist on either Mars or Venus although we are not able to see them.

Well, but are there men on Venus or Mars? some one may urge; and in true Scottish fashion I pass on the question and ask, What does the reader think?—Chambers's Journal.

A New Method of Observing N-Rays.—R. Blondlot describes a method in which, instead of observing the sulphide on a black ground, it is only necessary to watch the appearance or disappearance of a colored line on a background of the complementary color. A thin line of the sulphide is traced on a piece of white cardboard, and this is then exposed to the light of a gas flame shining through orange-colored glass. When the flame is turned high, the only thing seen is the uniform yellow surface of the cardboard. When it is turned very low, the blue color of the phosphorescence comes out clearly on the orange ground. The flame can be so regulated that the blue line only just disappears. If in this "sensitive" state the screen is exposed to N-rays, the blue line reappears, disappearing

again when the N-rays are stopped. All fatigue and strain of the eye, giving rise to the illusions specified by Lummer and Le Roux, are thus avoided. The same method may be used for the observation of the N'-rays. The flame is then regulated that the blue line is just visible, and the N'-rays then make it disappear. The author points out that these observations require a certain training of the eye, just as the ear requires training, according to Helmholtz, for the discovery of harmonics.—R. Blondlot, Comptes Rendus, July 11, 1904.

TREATING TELEGRAPH POLES.

For the last two years the Bureau of Forestry has been co-operating with the American Telephone and Telegraph Company and recently with the Postal Telegraph-Cable Company also, in an experimental study to increase the durability of telegraph and telephone poles. The interest in this matter taken by these corporations promises an important forest economy through the possibility of using much smaller trees than are now cut for poles. This means a new market for these smaller trees and liberating the larger ones for other uses.

The length of service of a telegraph or telephone pole is determined in a section of the pole not more than a foot or a foot and a half long. In a standing pole this section extends about six or eight inches above and below the top of the ground. This is the universal point of attack upon the life of the pole, and is called its breaking point. Decay is the arch-enemy of these poles. It sets in at the ground line and reaches both up and down the pole, but only so far as the conditions exist which promote the growth of wood-destroying fungi. A few inches below the ground there is lack of the necessary oxygen and heat, while at about the same distance above ground the requisite moisture fails. The exact time at which decay begins its work depends upon the climate, the character of the soil, and similar conditions. In a hot, moist climate it ordinarily sets in with great rapidity. But at best, in a very few years after the pole is set the struggle has commenced. The decay soon girdles the pole and gradually eats into it deeper and deeper until it is so weakened that it breaks under the weight of its equipment.

The strain upon the pole from wind pressure and the weight of its cross-arms and wires is calculated for the ground line. When the diameter of this ground line is constantly decreased, the strength of the pole is proportionately reduced, and it becomes only a question of time when the pole must fall. Chestnut and white cedar have been found, among available woods, most successfully to resist decay; but the life of the former is only from twelve to fifteen years, and of the latter ten to twelve years. The co-operative study of the Bureau is for the purpose of extending, if possible, this time.

The experiments already made by the Bureau show conclusively that poles can be subjected to a preservative treatment which insures materially lengthened service. This treatment consists in impregnating the wood with antiseptics which prevent the growth of the fungi that cause decay. The treatment of telegraph and telephone poles, when attempted at all in this country, generally has been applied to the whole pole, requiring the use of air-tight cylinders 100 feet long or more. In these the poles are subjected to live steam for some time, when a vacuum is created. Creosote is then run in and pressure applied to force it into the wood. Manifestly this is a laborious process. Yet for telegraph and telephone poles only about one foot of the entire length needs to be made immune from fungus. If this foot at the fatal ground line can be preserved from decay, the rest of the pole will take care of itself. Experiments will now be made in treating the butts of the poles for a distance of about 8 feet, thus carrying the antiseptics just beyond the zone of decay attack. The creosote method will be used and dead oil of coal tar forced through the butt of the pole.

The telegraph companies have made little use of preservative treatment. They employ millions of poles on their various lines, and it would be a tremendous economy to add even a few years of service to the life of each pole. But there will be another large saving both to them and to the forests through preservative treatment. To provide a good margin against decay, poles are now much larger than demanded by the strain upon them. It is expected that decay will quickly eat away a furrow around the pole at the ground line, and the diameter of the pole at that point is gaged to allow for this weakening process. When it is known that decay, in a certain number of years, cuts the diameter from perhaps 12 to 8 inches, and that below 8 inches the weakened pole falls, the course to be pursued is obvious. Antiseptics prevent, for the time of their effectiveness, the starting of decay, and thus permit at the outset the selection of an 8-inch diameter rather than a 12-inch. The 4 inches saved represent a tremendous difference in the size and age of trees used for poles. Both the companies and the owners of forests will be great gainers by this economy, with its shortening of the length of time necessary to grow a pole.

Another feature of the co-operative work will be treatment of cross-arms. The companies have been treating them, but report too much absorption in some cases and not enough in others. The Bureau will more carefully grade the different kinds of wood and treat each class separately. In this way it is expected to secure a more equal absorption and more satisfactory results.

THE

Scientific American Supplement.

PUBLISHED WEEKLY.

Terms of Subscription, $5 a year.

Sent by mail, postage prepaid, to subscribers in any part of the United States or Canada. Six dollars a year, sent, prepaid, to any foreign country.

All the back numbers of THE SUPPLEMENT, from the commencement, January 1, 1876, can be had. Price, 10 cents each.

All the back volumes of THE SUPPLEMENT can likewise be supplied. Two volumes are issued yearly. Price of each volume, $2.50 stitched in paper, or $3.50 bound in stiff covers.

COMBINED RATES.—One copy of SCIENTIFIC AMERICAN and one copy of SCIENTIFIC AMERICAN SUPPLEMENT, one year, postpaid, $7.00.

A liberal discount to booksellers, news agents, and canvassers.

MUNN & CO., Publishers,

361 Broadway, New York, N. Y.

TABLE OF CONTENTS.